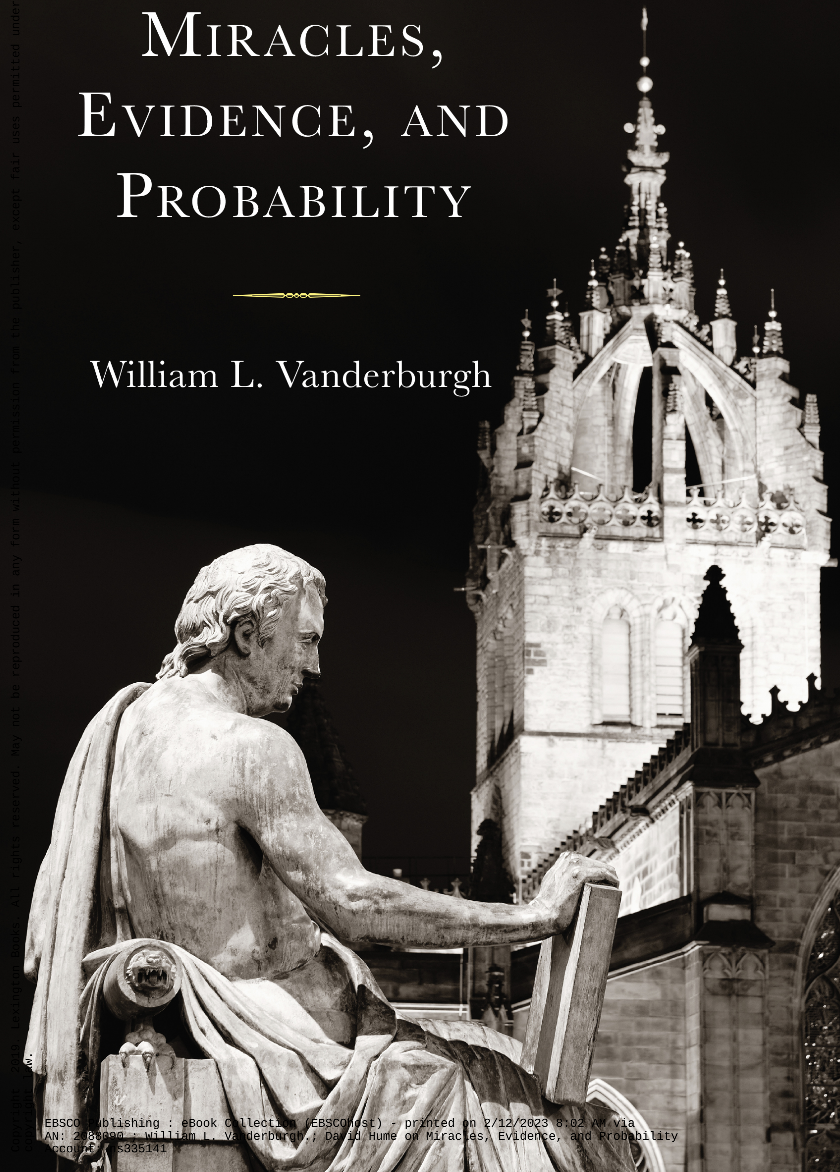
David Hume on Miracles, Evidence, and Probability

William L. Vanderburgh



Copyright 2019. Lexington Books. All rights reserved. May not be reproduced in any form without permission from the publisher, except fair uses permitted under U.S. or applicable copyright law.

EBSCO Publishing : eBook Collection (EBSCOhost) - printed on 2/12/2023 8:02 AM via
AN: 2088090 ; William L. Vanderburgh.; David Hume on Miracles, Evidence, and Probability
Account: ns335141

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

David Hume on Miracles, Evidence, and Probability

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

David Hume on Miracles, Evidence, and Probability

William L. Vanderburgh

LEXINGTON BOOKS
Lanham • Boulder • New York • London

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Published by Lexington Books
An imprint of The Rowman & Littlefield Publishing Group, Inc.
4501 Forbes Boulevard, Suite 200, Lanham, Maryland 20706
www.rowman.com

6 Tinworth Street, London SE11 5AL

Copyright © 2019 by The Rowman & Littlefield Publishing Group, Inc.

All rights reserved. No part of this book may be reproduced in any form or by any electronic or mechanical means, including information storage and retrieval systems, without written permission from the publisher, except by a reviewer who may quote passages in a review.

British Library Cataloguing in Publication Information Available

Library of Congress Cataloging-in-Publication Data

Names: Vanderburgh, William L., author
Title: David Hume on miracles, evidence, and probability / William L. Vanderburgh.
Description: Lanham : Lexington, 2019. | Includes bibliographical references and index.
Identifiers: LCCN 2019010973 (print) | LCCN 2019012410 (ebook) | ISBN 9781498596947 (electronic) | ISBN 9781498596930 (cloth : alk. paper)
Subjects: LCSH: Hume, David, 1711–1776.
Classification: LCC B1498 (ebook) | LCC B1498 .V36 2019 (print) | DDC 192–dc23
LC record available at https://lccn.loc.gov/2019010973

™ The paper used in this publication meets the minimum requirements of American National Standard for Information Sciences Permanence of Paper for Printed Library Materials, ANSI/NISO Z39.48-1992.

Printed in the United States of America

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Contents

Acknowledgments vii
A Note on Citations ix
Prologue: What This Book Is About 1

1 "Of Miracles" in Context 11
2 Testimony 33
3 Laws of Nature and Reports of Miracles 49
4 Hume and the History of Evidential Probability 75
5 Hume and the Bayesians 105
6 Resolving an Apparent Tension within Hume's Epistemology 129
7 Flew, Fogelin, Ferguson, and Fogelin 147

Epilogue 163
A Brief Biography of Hume 169
Hume's "Of Miracles": Chapter 10 of *An Enquiry concerning Human Understanding* 171
References 187
Index 193
About the Author 195

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Acknowledgments

This project had humble roots; I did not set out to write a book. This project began as a visceral reaction against John Earman's *Hume's Abject Failure: The Argument Against Miracles* (2000). I thank Eva Dadlez for inviting me to present a paper on that subject at the South Central Society for Eighteenth Century Studies (South Padre Island, February 2002). A revised version of that paper was presented at the Mountain-Plains Philosophy Conference (Las Vegas, October 2002), and at the Eastern Division of the American Philosophical Association (Philadelphia, December 2002). My commentators, Tim Costelloe (in Las Vegas) and Ted Morris (in Philadelphia), offered generous and helpful remarks. I presented related papers, which developed in more detail the material on the pre-Pascalian tradition of evidential probability, to the International Society for the History of Philosophy of Science (San Francisco, June 2004) and to the Canadian Society for the History and Philosophy of Science (London, Ontario, May 2005). The editors and referees of *Hume Studies* provided detailed and very useful feedback on my paper, "Of Miracles and Evidential Probability: Hume's 'Abject Failure' Vindicated" (2005). Parts of that work are reproduced here by permission, in a reworked form. It was as I was responding to those referee comments that I realized I was well on my way to having enough material for a book. I had an initial draft manuscript written by 2006, but various circumstances (including going through the tenure process and then a move to various full-time roles in academic administration) meant that the draft sat on my hard drive for far too long. However, coming back to it with fresh eyes has, I hope, made it a stronger book.

In addition to the friends and colleagues I thanked for their help with the *Hume Studies* paper, I am grateful to George Dehner, James Franklin, Dennis Hull, and an anonymous reviewer for this press for offering various comments and discussions that improved this book. Thanks also to Malcolm Johnson and Audrey Garrett for research assistance.

I would like to acknowledge Robert Muehlmann, a former editor of *Hume Studies*, whose 1994 graduate course on Hume was my introduction to a subject I quickly learned to love. Without that course and the excellent training in both the history of philosophy and the philosophy of science that I received at the University of Western Ontario, this book would not have been possible.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Most especially, I want to thank Sarah Brewer, who has provided constant love, support, and encouragement as I worked on this project—as she does in every aspect of our beautiful life together.

A funny story: The first time I gave a conference presentation on this topic, I got such terrible food poisoning that I had to stay in my hotel room for three days. The second time I presented on this topic, I got food poisoning again. Maybe God was trying to tell me something. But I don't think so. Hume's arguments are really solid. The inductive conclusion I'm drawing from this regularity of experience is to not eat the salad at conference hotels.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

A Note on Citations

Hume's *Enquiry concerning Human Understanding* and *Treatise of Human Nature* are cited respectively as "EHU chapter.paragraph" and "THN book.part.section.paragraph," from the Oxford editions first published in 1999 and 2000, respectively. Hume's *Dialogues concerning Natural Religion* is cited as "DNR part.paragraph." John Locke's *Essay Concerning Human Understanding* is cited as "Locke 1690, book.chapter.section(.paragraph), page," and quotations are taken from the 1975 edition by N.H. Nidditch. Other citations are by the "author (year, page)" method.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Prologue

What This Book Is About

This book is a philosophical and historical commentary on an important and continuing episode in the history of reasoning about evidence and probability, namely the debate over David Hume's argument that there is never sufficient evidence for rational belief in the occurrence of miracles.

Hume tells us that, "*I should not believe such a story were it told me by CATO*; was a proverbial saying in ROME, even during the lifetime of that philosophical patriot. The incredibility of a fact, it was allowed, might invalidate so great an authority" (EHU 10.9). His point is that some stories are too improbable to be believed, no matter the source of the testimony. That, in essence, is the conclusion of his argument against believing in miracles. There are, of course, some further details to consider.

David Hume's argument against miracles was controversial when it was initially published in 1748, and it has never ceased to be so. Theists of various stripes have tried to show that Hume's argument is flawed in order to establish, or at least make possible, the reasonableness of belief in miracles and the reasonableness of religious belief in general. Hume's argument has also had many champions. However, it is true that, "The many interpreters of Hume's essay, 'Of Miracles' . . . have so variously represented his views that the secondary literature muddles as often as it elucidates" (Bagger 1997, 237).

I contend that almost all these commentators, both pro and con, have missed important aspects of Hume's argument and have, crucially, misunderstood Hume's theory of probability and his use of evidential reasoning. This book aims to set the record straight about Hume's use of evidential probability in the argument against miracles, to clarify what he means by probability in general, and to show that Hume's account connects to other accounts of probability both before and after his writing. Doing this makes it clear that Hume's argument against miracles succeeds on its own terms.

One example of a work that gets Hume wrong on probability is *Hume's Abject Failure: The Argument Against Miracles* (2000), in which the eminent philosopher of science John Earman applies his considerable philosophical, technical, and rhetorical skills to "Of Miracles," the famous tenth section of David Hume's *Enquiry concerning Human Understanding*—and concludes that it is deeply flawed. Earman writes:

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> I contend that "Of Miracles" is an abject failure. It is not simply that Hume's essay does not achieve its goals, but that his goals are ambiguous and confused. Most of Hume's considerations are unoriginal, warmed over versions of arguments that are found in the writings of his predecessors and contemporaries. And the parts of "Of Miracles" that set Hume apart do not stand up to scrutiny. Worse still, the essay reveals the weakness and the poverty of Hume's own account of induction and probabilistic reasoning. And to cap it all off, the essay represents the kind of overreaching that gives philosophy a bad name. (Earman 2000, 3)

This is strong stuff. It is also, I think, almost entirely mistaken. Through historical work, textual scrutiny, and conceptual analysis, this book argues that Hume's case against miracles is in fact plausible in itself. Most of the criticisms leveled against it, including Earman's, fail to hit home because they misunderstand the aims and principles of Hume's epistemology in general or misconstrue the argument against miracles in particular, or both.

The goal in this book is not to provide a comprehensive analysis of all the literature on Hume on miracles—or even of any very extensive portion of it. The quantity of writing on Hume's "Of Miracles" long ago passed the point where such a review would be reasonable. There is, nevertheless, value to be had in a clear contextualization and explanation of Hume's position and in a critique of prominent, representative interpretations of it. For those who doubt the originality of the contribution Hume made in "Of Miracles," or its value, it is worth considering just how fruitful and important a single brief chapter (a mere seventeen pages in the 1999 Oxford edition edited by Beauchamp) has to be in order to spawn thousands of pages of passionate engagement that still continues today, over two hundred seventy years later.

A significant aspect of this book, in addition to its contribution to a central and perennial topic in history of philosophy, epistemology, and philosophy of religion, is its explanation of *why* so many commentators have fundamentally misunderstood Hume on miracles: No one before has noticed that Hume's theory of evidential reasoning and probable belief takes its structure and content from a tradition of probability that dates all the way back to ancient Roman law. Showing that Hume's account of probability has this structure and origin simultaneously demonstrates that the mathematical theory of probability—used by Earman and so many others to analyze Hume's argument against miracles—is in fact incommensurable with the approach to probability employed by Hume. Those analyses through the lens of mathematical probability theory end up being simply beside the point.

The fact that no previous authors have noticed the connection between Hume's theory of probability and the theory of evidence and proof in Roman law can be attributed in part to the hegemony that the mathe-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

matical theory of probability has enjoyed since soon after its development, which happened to be more or less at exactly the moment Hume's essay was published. The academic fields of history of philosophy and history of science were so beguiled by mathematical probability theory that they have forgotten the ancient tradition of evidential reasoning in which Hume participates. This means that, in essence, philosophers and historians have re-interpreted large parts of the history of ideas through the lens of the mathematical theory of probability, for the most part without noticing they were doing so. Eliminating this blind spot from the study of the history ideas—recovering the lost memory of an alternative way of thinking about evidential probability, so to speak—is crucial for a proper understanding of Hume. Perhaps even more importantly, it will also enable us to correct our views of the works of many other important thinkers (before, during, and even after the early modern period) who utilize this same ancient account of probability rather than the modern mathematical theory of probability. Rediscovering this ancient tradition may even provide resources for thinking about probability in contemporary contexts where mathematical probability simply does not seem to work.

Understanding Hume's argument against miracles is important, also, because that argument is a paradigm of the Humean approach to epistemology and to philosophy of religion. Many of his core themes are involved—from empiricism, skepticism, and reasonable judgment, to distrust of metaphysical and religious claims. Hume himself took it that a proper attitude toward religious belief was a fundamental part of having the right philosophical view of the world. Understanding Hume's view on religion in general, and his view on miracles in particular, is thus imperative for historians of philosophy—and for anyone who is concerned to explore what it means to be a reasonable, careful thinker. We cannot properly understand those views of Hume's except through his epistemology taken as a whole, and that includes his distinctive approach to probability that has largely been misunderstood or missed altogether.

THE ANCIENT TRADITION OF EVIDENCE AND HUME'S ARGUMENT

Hume's argument against miracles—more accurately, his argument to the conclusion that *there is never sufficient evidence for rational belief in the occurrence of miracles*—depends upon balancing the probabilities derived from different kinds of evidence. Hume's use of probability has seemed strange to many commentators; it has been considered the source of his mistaken conclusion by critics, and it has been a locus of confusion even for some of Hume's defenders. Beginning as early as Richard Price's essay, "On the Importance of Christianity and the Nature of Historical

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Evidence, and Miracles" (1767), commentators have attempted to critique or explicate Hume's comments on evidential probability in terms of the mathematical theory of probability. "The calculus of chances," as it was originally known, was first developed by Blaise Pascal and a close group of his associates beginning around 1654.

My defense of Hume's argument against miracles involves showing, following crucial work by L. Jonathan Cohen and by Dorothy Coleman, that Hume's account of probability and evidential reasoning is incommensurable with the mathematical theory of probability. This fact has been noted by commentators before, who then usually go on to claim that—therefore—Hume's account of probability and the argument against miracles that depends upon it are irredeemably flawed. My view is different. I think Hume's account of evidential probability is solid and that when it is understood on its own terms instead of being reinterpreted in a way that Hume did not intend, his conclusion about the incredibility of miracles will be seen to be correct.

The view that the mathematical theory of probability is the only acceptable way to think about probability is very widespread but, as I argue, there is a viable non-mathematical alternative. Hume's approach to evidential probability is firmly rooted in a tradition of probable reasoning that goes back to ancient Roman and medieval law. Besides having a long and successful history, this approach to probability has much to recommend it in many epistemic and evidential contexts. Because this ancient tradition of evidential reasoning is entirely *and deliberately* non-mathematical, interpretations of Hume that depend upon reading him through the lens of the mathematical theory of probability are fundamentally flawed, whether they are pro or con.

In illuminating the ancient theory of probability, I rely on an important book by James Franklin, *The Science of Conjecture: Evidence and Probability before Pascal* (2001). Franklin himself explicitly excludes Hume from the tradition he traces, perhaps because he is misled by the incorrect claims about Hume on probability that pervade the secondary literature on Hume. Part of my contribution is to show that Hume really does belong in the tradition Franklin traces.

Except for the connection between Hume's theory of probability and ancient Roman law, each of the strands of the story I tell here is known to historians of science, historians of philosophy, or philosophers of science. The strands are rarely, if ever, seen as related, however. As far as I have been able to determine, they have never been woven together before as they are here. The fact that the warp and weft of this story are not more widely known is itself a significant gap in our understanding of intellectual history. Hume's "alternative" approach to evidential probability was in fact the *dominant* approach throughout *most* of the history of western thought. If we neglect it we risk making serious mistakes in our interpretations of the history of science and the history of philosophy.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

To be clear from the outset, I do not claim that the non-mathematical approach to evidential probability is the best or the only tool for addressing problems of evidence in the contemporary sciences and other areas of life. I do believe that it is a plausible and useful framework, worthy of more consideration than it has been given. With explicit attention and development, it may provide valuable intellectual resources relevant to assessing significant contemporary debates such as those regarding Darwinian evolution and anthropogenic climate change. In the present work this idea is mostly implicit and programmatic, but I am confident of its potential.

THE PLAN OF THIS BOOK

After this prologue, chapter 1 ("Of Miracles" in Context) locates the argument against miracles within the context of Hume's thought. This sets the tone and delimits the scope of the subsequent discussion. Here I show both that (a) Hume's position on miracles flows naturally from his fundamental theory about how humans acquire knowledge of empirical facts, and that (b) his position on miracles dovetails perfectly with his general critique of religious belief. This part of the project shows that many commentators make mistakes about Hume's argument against miracles in virtue of misunderstanding central aspects of Hume's basic epistemology and philosophy of religion. I then outline the argument against miracles itself. Getting this right is important because common misinterpretations of parts of Hume's philosophy have led various commentators to incorrectly describe both the structure and conclusions of Hume's argument against believing in miracles. I give special attention here to Hume's definition of "miracle" and why various complaints about the cogency of that definition are mistaken. This provides an opportunity to introduce Hume's fundamental epistemology of empirical evidence, and how that relates to laws of nature and Hume's account of the probability of miracles.

Next, in chapters 2 and 3, I address in detail various individual components of Hume's argument against miracles. Chapter 2 ("Testimony") considers the factors that affect the reliability of testimony in general. I argue that Hume's account of the evidence of testimony has strong resemblances to John Locke's as well as to the account of testimony presented in Arnauld and Nicole's *Logic or the Art of Thinking*. I show that invoking the Principle of the Uniformity of Nature to evaluate the reliability of testimony does not make Hume's argument against miracles circular. Chapter 2 ends by answering an important challenge, namely understanding how it would even be possible in Hume's account for a well-established law of nature to be overturned by new evidence.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Chapter 3 ("Laws of Nature and Reports of Miracles") extends this discussion by considering Hume's account of how we acquire knowledge of the laws of nature. This includes delving into aspects of Hume's epistemology of laws of nature that get short shrift in typical accounts, for example what Hume thinks about imperfectly regular laws of nature, singular events, single-case-inductions, and purported exceptions to established laws of nature—all of which are relevant to the discussion of miracles. The second half of this chapter then dives into the details of Hume's approach to testimony regarding miracles in particular. This includes discussion of what Earman calls Hume's Maxim and Hume's Diminution Principle.

In addition to demonstrating historical antecedents to Hume's positions (for example, Locke's account of testimony and his account of miracles), chapters 2 and 3 provide context for and details of Hume's argument against miracles that build upon the analysis begun in chapter 1. Some important criticisms leveled against Hume's argument—including, for example, the charge that his definition of "miracle" leads to circularity, and the charge that Hume's argument is a priori rather than empirical in that it seems to rule out the discovery of new laws that overturn previously held laws of nature—are answered in detail in chapters 1 through 3. As befits a skeptical empiricist of Hume's stripe, his ultimate conclusion about miracles is an *epistemic* one: Given what we know about human fallibility in perception, memory, and testimony, the total weight of evidence in favor of the claim that a violation of a law of nature has occurred will never, practically speaking, be sufficient to over-balance the weight of evidence in favor of the counter-claim that the laws of nature are uniform. Chapters 1 through 3 are largely exegetical, then, but with an argumentative point: Once we clearly understand "Of Miracles" and its historical, philosophical, and theological context, we see that many of the criticisms leveled against it misfire.

Chapter 4 ("Hume and the History of Evidential Probability") discusses Hume's general theory of empirical probability, including how it differs from the mathematical account of probability and how it fits into the history of reasoning about probable evidence. Here I follow Coleman (2001), who shows that Hume's approach to evidential probability has an entirely different structure and basis than does the mathematical theory of probability. Then, following Franklin (2001), I give a précis of the history of non-mathematical probability from its origins in ancient Roman and medieval law up through early modern philosophy and science. I show that this history maps exactly onto the "Baconian" (as opposed to "Pascalian") approach to probability that Coleman (2001) attributes to Hume. Then, I show where Ian Hacking's older but influential work, "Hume's Species of Probability" (1978), goes wrong in its account of Hume's theory of probability. This is an opportunity to expand upon some of the themes taken up earlier in the book and to further contextual-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

ize Hume in the history of reasoning about probability. In particular, I explore ancient antecedents of Hume's mitigated skepticism that are important to fully understanding his views about probable empirical reasoning.

In chapter 5 ("Hume and the Bayesians"), I survey some of the Bayesians's more serious errors of interpretation with regard to Hume, focusing particularly on a common but incorrect claim that Hume takes the probability of a miracle occurring to be "flatly zero"; this is closely related to a common misunderstanding of what Hume means by "proof." On the latter point, many commentators have failed to fully understand Hume's threefold distinction between probabilities, proofs, and demonstrations. To foreshadow: We have a proof when an empirical generalization has such strong evidence that there is no basis for reasonable doubt—a degree of belief early modern philosophers call "moral certainty." This is a standard not equivalent to absolute certainty, just as "not guilty" is not equivalent to "innocent" in courts of law. Contrary to what many of his critics have suggested, Hume does not think that his *proof* against miracles establishes the *impossibility* of the existence of miracles. Rather, Hume thinks that the available evidence gives such a high degree of probability to the laws of nature that belief in the existence of miracles can never be rational—that is, sufficiently well-grounded epistemically. Hume's conclusion is thus much more nuanced and moderate than many commentators have supposed. I also here examine a set of problems related to the issue of whether or not a non-Bayesian account of evidential probability is conceptually viable. I argue that it is. One important component of this is my answer to a likely Bayesian challenge to the non-Pascalian approach, one that is based on the so-called Dutch Book argument. Bayesians often contend that the Dutch Book argument shows that reasoning in accordance with the axioms of mathematical probability theory is a "condition of rationality" (failing to reason that way makes you fundamentally irrational). I contend that the Dutch Book argument merely assumes the Pascalian scale of probability rather than proving that the Pascalian approach is the only viable one, and I argue for a contextualist, pluralist account of probability according to which different ways of treating probability should be employed in different kinds of problem situations.

Chapter 6 ("Resolving an Apparent Tension within Hume's Epistemology") addresses the question of whether, given his other philosophical commitments, Hume is entitled to his normative conclusions regarding belief in miracles. The question arises because on Hume's account it is an inescapable fact of human nature that when making inferences from past evidence to empirical generalizations (for example, bread has nourished us in the past, so it will always nourish us in the future), we crucially rely on *arational* habits of the mind. Given this, it is necessary to show that Hume is not committed to the view that *whatever* we might

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

believe after having a given body of experience is *the right thing* to believe given that experience. To put this another way, the worry is that since Hume holds that belief formation is in a strong sense rationally unjustifiable, it may be inconsistent for him to claim that some beliefs are rationally or evidentially better than others. I argue that Hume can successfully answer this worry and thus that his normative stance against believing in miracles is, in fact, consistent with his other philosophical commitments.

Chapter 7 ("Flew, Fogelin, Ferguson, and Fogelin") first gives an account of an exchange in the pages of the journal *Hume Studies* about whether Hume's argument concerning miracles is in any way a priori. I show that the debate turns on a misunderstanding of what Hume means by "proof" in his formula "proof against miracles." Once "proof" is properly understood, it is abundantly clear that Hume's argument is not a priori. I thus correct both sides in the Flew/Fogelin debate, at the same time clarifying the aims and structure of Hume's argument in the face of common misconceptions about it. Next, I discuss Robert Fogelin's (2003) *A Defense of Hume on Miracles*, which is a direct response to Earman (2000). Fogelin (2003) and I share much in common in our views about Hume and in our criticisms of Earman, but there are also some significant differences between us. Fogelin, for example, does not see the role of non-Pascalian probability in Hume's epistemology, and as a result goes so far as to call Hume a proto-Bayesian—something I think is a radical mistake. I focus particularly in this section on Fogelin's response to Earman's claim that Hume adopts the "straight rule" of induction. I agree with Fogelin that Earman is wrong to attribute the straight rule to Hume (Hume's view about induction is much more sophisticated than that), but I argue that Fogelin has given a fundamentally incorrect explanation of why this is so.

The Epilogue sums up the book while exploring implications of the account offered here for future work in the history of ideas, the philosophy of science, and especially Hume scholarship. Once understood within the framework of pre-Pascalian probability and in the context of Hume's thought generally, Hume's argument against miracles is seen to be both plausible in its own right and immune to many of the criticisms leveled against it.

The appendix to this work includes a brief biographical account of Hume and reprints "Of Miracles," chapter 10 of Hume's *Enquiry concerning Human Understanding.*

Overall, then, this book aims to do main five things. First, it gives a clear statement of the structure and conclusions of Hume's argument against believing in miracles, carefully putting it into historical and philosophical context. Second, it provides a definitive defense of Hume from Bayesian attacks, in part by correcting those instances in which Fogelin's answers to Earman go awry, and in part by revealing additional errors in Earman's and other Bayesian accounts of Hume. Third, this book devel-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

ops Coleman's point that Hume is a non-Pascalian about probability. Fourth, it draws attention to the ancient tradition of probable reasoning by commenting on Franklin's work, and shows that Hume is part of the tradition Franklin illuminates; part of the contribution here is to connect Hume to a tradition of probability that is much older than Coleman realized. Fifth, this book offers a partial conceptual defense of the non-Pascalian approach to evidential probability, also suggesting its plausibility even today as a framework for understanding evidence.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

ONE
"Of Miracles" in Context

THE THEOLOGICAL CONTEXT OF "OF MIRACLES"

This chapter describes some of the context in which Hume wrote about miracles and the positions he was reacting against. It briefly considers Hume's ultimate philosophical stance regarding religious belief and examines some common errors commentators have committed and misunderstandings they have held that have led them to make incorrect claims about Hume's argument. Subsequent chapters go into more detail on the argument against miracles itself, Hume's epistemology, Hume's theory of probable reasoning, and other factors that help lay out a correct view of what Hume was attempting and what he achieved in his attack on belief in miracles.

The essay "Of Miracles" seems to have existed in some form while Hume was composing his *Treatise of Human Nature* (first published 1739-40). Probably for fear of upsetting religious authorities, Hume did not include it in the *Treatise*. Hume did, however, send a copy of the early version to his cousin Henry Home ("Home" is the old spelling of Hume's family name), asking him to destroy the manuscript after reading it; only the letter to Henry (dated 2 Dec. 1737) survives (Mossner 1980, 112):

> [I] enclose some Reasonings concerning Miracles, which I once thought of publishing with the rest, but which I am afraid will give too much offence, even as the world is disposed at present. . . .Your thoughts and mine agree with respect to Dr Butler and I would be glad to be introduced to him. I am at present castrating my work, that is, cutting off its nobler parts; that is, endeavouring it shall give as little offence as possible, before which, I could not pretend to put it in the Doctor's hands. This is a piece of cowardice, for which I blame myself, though I believe none of my friends will blame me. But I was resolved not to be an

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> enthusiast in philosophy, while I was blaming other enthusiasms. (Grieg 1932, letter 6)

Hume's argument against miracles thus makes its debut in the *Enquiry concerning Human Understanding* (first edition 1748), and it undergoes some minor revisions in later editions of the *Enquiry* (1768 and 1777). Hume's remarks about miracles fit together with other aspects of his critique of religion and with his philosophy generally. Let me explain how.

J.C.A. Gaskin (1993, 318) points out that for Hume's contemporaries, Christian apologetics, or the discipline concerned with justifying Christian religious belief consists of two parts. The first part is appeal to reason, specifically to arguments either a priori or a posteriori. These a priori and a posteriori rational arguments constitute "natural religion" or religion as discoverable by "the natural light," reason. The second part of apologetics involves appeal to revelation. I will consider each of these two parts of the apologetic project briefly in turn, in order to put Hume's argument against miracles, and his general critique of religion, into context.

The theological attempt to provide rational justification for religious belief goes back at least to Aquinas's "The Five Ways." The paradigmatic case of the argument a priori is the Cosmological Argument: Everything must have a cause, therefore the world itself had a cause—where the cause is itself a necessary being and therefore uncaused. Although it may sound odd to twenty-first-century ears to call the Cosmological Argument "a priori" since it begins from apparently empirical (i.e., a posteriori) facts about causation, Hume's usage here is consistent with that of his contemporaries. As Gaskin (1993, 314–15) explains with reference to Hume's "paraphrase [at DNR 9.3] of a particular cosmological argument to be found in Samuel Clarke's Boyle Lectures for 1708," in virtue of classing the Cosmological Argument as a priori Hume is emphasizing the *universality* of the metaphysical claim made in the causal premise and the fact that the conclusion involves a *necessary* being—universality and necessity being two things that cannot be known a posteriori according to Hume's epistemology and which hence must be a priori. Several of the other standard arguments for the existence of God (the Ontological Argument, Descartes's arguments in the *Meditations*, and so on) would also fall into the a priori component of natural religion.

The paradigmatic case of the a posteriori argument is the design argument: The parts of the world are well-fitted to each other; in artificial objects, "well-fittedness" is the result of conscious design, not accident; therefore, by analogy, the world must have had an intelligent designer.

After this two-part appeal to reason, the second part of Christian apologetics among Hume's contemporaries involves appeal to revelation, specifically to the revelation of the New Testament, the authenticity of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

which was claimed to be supported by the fulfilled prophecies and the miracles that attend the revelation in question. The guiding inference rule here is that, since only the one true God can know the future and perform miracles, any purported revelation that is attended by correct prophecies and actual miracles must be a true divine revelation. The prophecies and miracles function, then, like a stamp of official divine endorsement of the doctrines revealed (kind of like American political ads: "I am God and I approve this message").

Hume's critique of religion, while it is spread over several works, attacks each part of this traditional religious apologetic strategy. Hume's overall position may be summarized this way: "arguments of natural religion do not establish the existence of any deity which could be an object of religious belief, and . . . revelation is not authenticated in any way that could convince a rational [person]" (Gaskin 1993, 319). If Hume is right, then there is no rational justification for religious belief. To put it another way, there are no *reasons* for belief, only *causes* (be they psychological, sociological, historical, or other). Hume's attack on the arguments of natural religion is mounted in the *Treatise of Human Nature,* the *Enquiry concerning Human Understanding*, and the *Dialogues concerning Natural Religion*. His *Natural History of Religion* then examines the causes of belief. (Falkenstein 2003 presents an interesting case about where NHR fits in Hume's overall critique of religious belief, but I won't discuss that in detail here.) Section 10 of the *Enquiry* conducts the attack on miracles as a rational ground for religious belief. Note that Hume correctly treats prophecy as a species of miracle (see EHU 10.41): No natural process can allow humans to know the future, thus if they do so it must be through a supernatural process and in violation of the laws of nature. It is worth noting that Harrison (1999) argues that Hume is wrong to count prophecy as a species of miracle. Though I disagree—humans knowing the future certainly seems to violate laws of nature—since I am not here offering a complete analysis of Hume's entire epistemology of religion (for that see Gaskin 1988), this is an issue we can bracket for present purposes. Hume discusses other aspects of the philosophy of religion in his essays "Of Suicide," "Of Superstition and Enthusiasm," "On the Immortality of the Soul," and in Section 11 of the *Enquiry*, "Of a particular Providence and of a future State." It is worth remembering that for Hume, getting the right position on religion is important: "Generally speaking, the errors in religion are dangerous; those in philosophy only ridiculous" (THN 1.4.7.13).

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

INCONCLUSIVE REMARKS ON HUME'S OWN RELIGIOUS POSITION

Although "Of Miracles" is part of Hume's devastating critique of the credentials of religious belief, instead of concluding that religious beliefs are *false*, Hume remarks that "Our most holy religion is founded on *Faith*, not reason" (EHU 10.40). Gaskin suggests that this is not a sincere defense of fideism, which is the view that "religion is founded on faith alone, not reason, because every proposition rests on premises accepted 'on faith'" (Gaskin 1993, 320). Hume's statement is then perhaps ironic. Unsurprisingly for this sort of interpretive problem, there are competing explanations. On related matters Peter Fosl argues that Hume considers the "categories of atheism, dogmatic faith, and even agnosticism to be an incomplete set of alternatives" (Fosl 1994, 111), and thus Hume's overall religious position is a kind of mitigated theism. Hume's remark at the end of "Of Miracles" could then be a sincere statement of his position. Examining in detail all of those competing explanations of Hume's overall religious position would take too much space here, though I do offer some hints in the following paragraphs. In any case, the essential point for the purposes of the present chapter is made by the quotation that follows below.

Though Hume's arguments do appear to tend toward the skeptical and even the atheistic, it is important to consider that Hume is not free, in his cultural context, to explicitly avow atheistic sentiments. Hume may simply have been protecting himself with the nod to orthodoxy mentioned above in which he says that Christianity is founded on faith rather than reason. This reading is certainly reinforced by the passage with which Hume concludes "Of Miracles":

> [U]pon the whole, we may conclude, that the CHRISTIAN religion not only was at first attended with miracles, but even at this day cannot be believed by any reasonable person without one. Mere reason is insufficient to convince us of its veracity: And whoever is moved by *Faith* to assent to it, is conscious of a continued miracle in his own person, which subverts all the principles of his understanding, and gives him a determination to believe what is most contrary to custom and experience. (EHU 10.41)

Hume is here expressing the idea, in the harshest terms possible given his epistemology and his social milieu, that to assent to Christianity (or to any religion, for that matter) purely on the basis of faith is to perversely and irresponsibly reason improperly—more exactly, it is to abandon reason. Rational belief, as Hume will be seen below to hold, must be adjusted to the available evidential warrant; faith, by definition, neither is nor has any sort of warrant. Irrational belief is its own indictment.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Hume's critique of religion is thorough-going and complete. As Gaskin (1993) notes, however, Hume is such a consistent skeptic that he does not definitely *deny* the existence of God or the truth of Christianity, even though he thinks the evidence is much less than what would be required for rationally justified belief. The only positive religious claim Hume thinks well-founded is that *something* in the design argument survives critique—but note how carefully he qualifies this claim:

> If the whole of natural theology, as some people seem to maintain, resolves itself into one simple, though somewhat ambiguous, at least undefined, proposition: *That the cause or causes of order in the universe probably bear some remote analogy to human intelligence:* If this proposition be not capable of extension, variation, or more particular explication: If it affords no inference that affects human life, or can be the source of any action or forbearance: And if the analogy, imperfect as it is, can be carried no further than to the human intelligence, and cannot be transferred, with any appearance of probability, to the other qualities of the mind: If this really be the case, what can the most inquisitive, contemplative, and religious man do more than give a plain, philosophical assent to this proposition, as often as it occurs, and believe that the arguments on which it is established exceed the objections which lie against it? (DNR 12.31)

Earman refers to this passage from the *Dialogues*, and he gets Hume's overall position correct, except in two important respects. I deal with the second respect in the following section. The first respect in which Earman mistakes Hume's position is that Earman says "the mature Hume was a theist, albeit of a vague and weak-kneed sort" (2000, 4). This is a remarkably unfair way to put it, since it both overstates the content of Hume's religious position and insinuates that the position being attributed to Hume somehow is wrong or reveals a character flaw in Hume. A better way to describe Hume's religious position is to follow Gaskin and call it "attenuated deism" (Gaskin 1988, 219–29, see especially 223). Hume grants, at most, that reason informs us that there is some intelligent creator, but he forcefully denies that we have adequate grounds to think this deity has the characteristics normally attributed to God by theists. Moreover, Hume insists that there is no reason to think the deity is at all interested in human lives. (See O'Connor 2001, 10–11.) Such a position is hardly "weak-kneed" since holding it would have required a great deal of bravery in Hume's cultural context. Hume's religious position was, in fact, the result of a nuanced, rigorous intellectual process that Hume had the fortitude to follow to its logical end and stick to despite the tenacious psychological inclinations and unrelenting social pressures to which many people surrender.

In *The Natural History of Religion*, first published in 1758 and revised by Hume through to the posthumous 1777 edition, Hume appears to make the claim even more strongly that we can reasonably infer an Intel-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

ligent Designer from the order in the world. At the beginning of the work, he writes, "The whole frame of nature bespeaks an intelligent author; and no rational enquirer can, after serious reflexion, suspend his belief a moment with regard to the primary principles of genuine Theism and Religion" (Hume 2013 [1777], 439). This claim seems quite strong, but Hume is actually being cagey: His understanding of "genuine Theism and Religion" is much different than he knows it will be for most of his contemporary readers. This minor piece of misdirection is aimed at drawing in readers who would be offended by a bald statement of deism or atheism, allowing them to be slowly brought to agree with Hume's position through his analysis of the merely-psychological causes of religious belief. As Hume puts it in *The Natural History of Religion*, there are causes of belief arising from *human nature* rather than from *reason*. Hume eventually ends with the same conclusion we saw in the *Dialogues concerning Natural Religion*:

> [I]t scarce seems possible, that any one of good understanding should reject the idea [that there is a sovereign author of the works of nature], when once it is suggested to him. A purpose, an intention, a design is evident in every thing; and when our comprehension is so far enlarged as to contemplate the first rise of this visible system, we must adopt, with the strongest conviction, the idea of some intelligent cause or author. (Hume 2013 [1777], 503)

But this apparently strong position gets curtailed in the final paragraph: "The whole is a riddle, an aenigma, an inexplicable mystery. Doubt, uncertainty, suspence of judgment appear the only result of our most accurate scrutiny, concerning this subject" (Hume 2013 [1777], 505). Hume leaves us here just where he left us in the *Dialogues concerning Natural Religion*. We must take this, then, to be his considered and honest opinion. Hume accepts the inference from the apparent design in the world to the existence of some cause of the world that we could call an Intelligent Creator, but reason tells us nothing else about the nature or properties of this being or force. It is certainly not an anthropomorphic being, and there is no sign it is concerned with anything we do.

Dees (2002) pursues an alternative explanation of the concession made by Philo at the end of the *Dialogues*. Dees catalogues the various interpretations that have been offered—as treating Philo (Hume) as insincere, as making an empty concession for rhetorical purposes, as making a meaningless concession, or as making a sincere statement of unorthodox religious faith (Dees 2002, 132). Dees's own view is that in the context of the *Dialogues* the concession reflects the importance Philo (Hume) places on good manners and sociability: Morality is more important than metaphysics, and Philo makes the concession that something in the design argument survives critique because he wishes to re-establish bonds of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

friendship with Cleanthes after his too-vigorous anti-theistic statements in Part 1 of the *Dialogues*.

I grant that this is a possible explanation, but it would require Hume to have taken his fictional world extremely seriously in order for him to be concerned enough with this point to allow it to skew the conclusion of his very carefully constructed *Dialogues*. The account I suggest above with respect to the argument against miracles (namely, that Hume is protecting himself from a charge of atheism through an innocuous nod to orthodoxy) seems more plausible to me in this case as well. Note, too, that a Dees-style view cannot possibly account for the way in which Hume ends the chapter "Of Miracles" or how he ends the *Natural History of Religion*, since obviously there is no context there of a personal relationship in need of preservation and maintenance. As I will show, however, Hume's emphasis on good manners and sociability does play a role later in his response to Richard Price's critique of "Of Miracles."

One commentator has described the range of interpretations of Hume's philosophy of religion this way:

> At one extreme are those who see Hume as an "atheist" or "anti-theist." At the other extreme are those who see Hume as some kind of theist, though not a classical or orthodox one. In between are others for whom Hume is an "agnostic" or "ironic skeptic." Still a fourth interpretation can found, according to which Hume "seems to vacillate hopelessly" in his view of religion. (Andre 1993, 141)

Andre argues for the claim that Hume is an unorthodox theist, in part because "atheist" implies at least that someone actively disbelieves that God exists. Hume saw the evidence for the orthodox theistic God, including the Judeo-Christian one, as sorely lacking, but this is not the same as Hume thinking the evidence is in favor of no deity existing. For example, Hume writes in *The Natural History of Religion* that

> The only point of theology, in which we shall find a consent of mankind almost universal, is, that there is an invisible, intelligent power in the world: But whether this power be supreme or subordinate, whether confined to one being, or distributed among several, what attributes, qualities, connexions or principles of action ought to be ascribed to these beings; concerning all these points, there is the widest difference in the popular systems of theology. (Hume 2013 [1777], 452)

Hume argues that this disagreement, as we will see in the case of "contrary miracles" discussed below, means that on balance the rational reasons for supporting any one religious hypothesis over any other are essentially nonexistent.

Thomas Holden coins the name "moral atheism" to denote "Hume's rejection of the existence of a deity with moral attributes":

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> In Hume's view, no first cause or designer responsible for the ordered universe could possibly have moral attributes; nor could the existence (or non-existence) of such a being have any real implications for moral practice or human life. . . . [Hume] is not a skeptic or agnostic on the question of the deity's moral character: he does not simply suspend judgment on regarding [divine] moral attributes, but categorically rules them out. (Holden 2010, ix)

My reading of Hume exactly concurs with Holden on these points. Whether Hume believes or does not believe in the existence of some sort of divinity, it is clear that he believes any such divinity has and should have no impact on human morality or forms of life.

Defining "deism" in general terms as belief in the existence of a God on purely rational grounds without reliance on revelation or authority, Gaskin's "attenuated deism" label seems to fit Hume's explicit positions. Note, though, that some critics, having a more specific and detailed version of deism in mind, argue that Hume was not a deist (see for example Fosl 1999, especially 174). I will not dispute about a word; call Hume's position "attenuated theism" or "limited theism" (Andre 1993, 142, 153) if you like. For the sake of this book, not much hangs on the minute details of how to categorize Hume's overall religious position. For the record, my own interpretation is closer to that of Gaskin or Andre than to those who see Hume as an outright atheist.

Whatever name we give to Hume's position on religion, it is fully in harmony with his general mitigated skepticism. Moreover, it is deliberate, carefully argued, and strongly held: Contrary to Earman's accusation, then, there is nothing weak-kneed about it. Hume had to be a man of strong conviction to stick to his guns and publicly proclaim a nonorthodox view when he could have simply stayed silent on the issue, and he suffered professionally for it (for example, he did not get appointed to the professorship he wanted).

IS HUME'S ARGUMENT AGAINST MIRACLES A PRIORI OR A POSTERIORI?

A second respect in which Earman misreads Hume's position on religious matters is a fundamental one, and it is an example of a common misreading. Earman claims that Hume's "Of Miracles" is "an in-principle attack on the possibility of establishing the credibility of religious miracles" (Earman 2000, 5). On the contrary, as I show, Hume's argument against believing in miracles is not an "in-principle" argument against the very possibility of miracles (it is not like a "demarcation criterion" in philosophy of science) but is rather a careful a posteriori assessment of the total state of the evidence in the light of human capacities and knowledge. The conclusion of Hume's argument is that, as a matter of fact, it is

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

extremely unlikely that we will ever obtain evidence for the occurrence of a miracle that is sufficient for rational belief. This sort of careful empirical analysis, where facts lead the theory, is just the sort of thing Earman and other contemporary empiricists think of as good philosophy.

It may reasonably be asked whether it is fair to assimilate "in-principle" with "a priori" and thus charge Earman with thinking Hume's argument is a priori when it is really a posteriori. It is true that Earman is not entirely clear or consistent on whether he thinks Hume is giving an a priori or an a posteriori argument in "Of Miracles." My answer is that *an in-principle attack on a possibility* aims to establish *the in-principle-impossibility of that thing*. This must mean logical impossibility (rather than, say, physical or practical impossibility), or else the phrase "in principle" is doing no work. Logical impossibilities cannot be proved a posteriori; therefore, it is fair to say that Earman thinks Hume is giving an a priori argument against the possibility of miracles. Attributing this a priori interpretation of Hume to Earman is supported, furthermore, by the fact that Earman explicitly talks about Hume making the probability of the occurrence of a miracle "flatly zero" (Earman 2000, 13, and throughout). In the mathematical theory of probability Earman uses to analyze Hume, there is no way to understand Pr(p) = 0 (which is read as, "the probability of the truth of the proposition p is zero") except as saying that the statement p is a logical impossibility. Instead of saying that Hume is conducting "in-principle attack on the possibility of establishing the credibility of religious miracles," we should more accurately and more simply say that he is conducting an attack on the credibility of miracles.

With this initial contextualization and explication of Hume's argument against believing in miracles in hand, we can see that various kinds of common objections to Hume simply miss the point. For example, in *The Everlasting Check: Hume on Miracles*, George (2016) raises a possible way of sidestepping Hume's argument that he finds in Paley's *A View of the Evidences of Christianity* (1794): "once believe that there is a God, and miracles are not incredible" (George 2016, 70, quoting Paley 1794, 15). The argument, advocated in detail by George (68–74), is essentially that if one already believes in the theological doctrines, then miracle reports thereby gain credibility, such that some miracles reports can then be rationally believed to have occurred. But the conditional statement, "If Christianity is true, then miracles occur," is either trivially tautologous (since Christianity includes the possibility and actuality of deity-sponsored or deity-performed violations of laws of nature) or it has an antecedent that Hume would not grant. *Of course* if you already fully accept the Christian doctrine, you (thereby) accept the miracles. By Hume's lights that is not philosophically interesting, since then miracles are not being used as a rational foundation for religious belief, but in some other way (self-congratulation, or something). What he would then be curious to critique is why you already believe and, as outlined above,

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

his critique of natural religion shows that there are no rational foundations for such belief by any route in the traditional framework of religious apologetics.

DEFINING "MIRACLE" AND THE BASIC STRUCTURE OF HUME'S ARGUMENT

We are now ready to begin examining in detail Hume's argument against believing in miracles. He writes,

> Nothing is so convenient as a decisive argument of this kind, which must at least *silence* the most arrogant bigotry and superstition, and free us from their impertinent solicitations. I flatter myself, that I have discovered an argument of like nature, which, if just, will, with the wise and learned, be an everlasting check to all kinds of superstitious delusion, and consequently, will be useful as long as the world endures. (EHU 10.2)

The goal over the next few sections is to show what the aims and parts of Hume's argument against miracles are, and to connect that argument to fundamental principles of Hume's epistemology and theory of evidence. Failing to attend to this context and these details has too often led commentators to make incorrect claims about Hume's position on miracles. Readers may wish to read "Of Miracles" itself before (or after) reading this chapter (it is reprinted as an Appendix to this volume).

"Of Miracles" was a lightning rod for criticism and debate in Hume's own time, and it has never ceased to be one. For those who wish to explore the historical controversy, Garrett (2002, especially 301–304 and associated footnotes) gives a nice summary of some of the major interpretations and critiques of Hume's essay. Earman (2000) does a good job of canvassing the eighteenth-century debate (especially the parts relevant to the development of mathematical probability theory), and the second half of Earman's book is an anthology of some of the most important sources for, and responses to, Hume's argument. Tweyman (1996) is a collection of some of the most important responses to Hume on miracles in the period 1751 to 1882.

The thesis Hume defends in "Of Miracles" is that reports of miracles, even eyewitness reports, are never adequate grounds for belief that a miracle has in fact occurred; even more importantly for Hume's purposes, such reports are never adequate grounds from which to infer the truth of a particular religious hypothesis.

In order to see how Hume reaches this conclusion, we should begin by being clear about what he means by a "miracle." Hume gives two complementary definitions, the first of which says: "a miracle is a violation of the laws of nature" (EHU 10.12). In a footnote to the same paragraph, Hume offers his second definition: "A miracle may be accurately defined,

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

a transgression of a law of nature by a particular volition of the Deity, or by the interposition of some invisible agent" (EHU 10.12, n. 13). It is important to understand that, contrary to initial appearances, Hume's discussion of miracles is not ontological but is, rather, epistemological. The issue is not whether *miracles can occur*, but whether *we can have adequate grounds for* believing *that miracles have occurred*, given everything we know and the way the human mind works.

It is worth remarking that Hume allows that miracles in his sense may be discoverable by humans or not—that is, Hume allows the possibility of "invisible miracles": "The raising of a house or ship into the air is a visible miracle. The raising of a feather, when the wind wants ever so little of a force requisite for that purpose, is as real a miracle, though not so sensible with regard to us" (EHU 10.12 n. 23). If Hume's argument against miracles succeeds in showing that we never have adequate empirical grounds to accept the occurrence of a miracle, the same conclusion of course holds for miracles that we cannot see or did not know occurred.

Hume likely had the doctrine of transubstantiation in mind as one example of an invisible miracle. The doctrine of transubstantiation holds that during the Communion ceremony, the bread and wine *literally* turn into the body and blood of Christ, even though there are no changes in the observable properties of those foodstuffs. In the *Treatise*, it is clear that Hume does not think highly of the doctrine of transubstantiation when he remarks, in a section about promise-making, that "since every new promise imposes a new obligation of morality on the person who promises, and since this new obligation arises from his will; 'tis one of the most mysterious and incomprehensible operations that can possibly be imagin'd, and may even be compar'd to *transubstantiation* . . . where certain forms of words, along with a certain intention, changes entirely the nature of the external object" (THN 3.2.5.14). The analogy is somewhat inexact, which leads a reader to think that Hume's point was really to mock transubstantiation rather than to say something about promises.

Evidence that Hume had transubstantiation in mind in the passage from "Of Miracles" that was mentioned above can be found in the fact that Hume refers to Tillotson's argument against the "real presence" (transubstantiation) in the very first sentence of "Of Miracles" (EHU 10.1–2). Hume thinks he has come up with an argument against miracles that is as a concise, elegant, and strong as Tillotson's. Tillotson's argument has to do with the fact that our evidence for transubstantiation comes from the testimony of the disciples, which testimony must diminish in probability through time, to the point that it now cannot overbalance the testimony of our senses to the contrary. Mentioning Tillotson's argument thus serves as a précis and foreshadowing of Hume's own argument against miracles, with which it shares significant structure and reasoning patterns.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Bagger follows Stewart (1995) in tracing the connection here to a different source in Tillotson's writings, where

> Tillotson argues that those who defend transubstantiation undermine the grounds for their position. The authority and doctrine of the Church rely on the apostles' testimony about the evidence of their senses. They report miracles which confirm Christ's divinity. The doctrine of transubstantiation, however, requires that one suspend one's reliance on the senses. One must believe that bread and wine, which to all sensory appearance remain bread and wine, actually become flesh and blood. (Bagger 1997, 239)

Then, the problem is that insisting on transubstantiation makes sensation sometimes unreliable, and thus undermines the apostles's testimony to other (visible) miracles. This is an interesting argument that I suspect Hume would be sympathetic to, but it does not match up very well with Hume's own description of the argument of Tillotson he invokes:

> There is, in Dr. Tillotson's writings, an argument against the *real presence*, which is as concise, and elegant, and strong as any argument can possibly be supposed against a doctrine, so little worthy of a serious refutation. It is acknowledged on all hands, says that learned prelate, that the authority, either of the scripture or of tradition, is founded merely in the testimony of the apostles, who were eye-witnesses to those miracles of our Saviour, by which he proved his divine mission. Our evidence, then, for the truth of the Christian religion is less than the evidence for the truth of our senses; because, even in the first authors of our religion, it was no greater; and it is evident it must diminish in passing from them to their disciples; nor can anyone rest such confidence in their testimony, as in the immediate object of his senses. But a weaker argument can never destroy a stronger; and therefore, were the doctrine of the real presence ever so clearly revealed in scripture, it were directly contrary to the rules of just reasoning to give our assent to it. (EHU 10.1)

It seems clear that this passage from Hume does not resemble in sufficient detail the other passage from Tillotson mentioned by Bagger (quoted immediately above). Therefore, that other passage cannot plausibly be the source of Hume's argument as Stewart and Bagger assert. As we shall see, this passage from Hume just quoted also serves as a quite complete summary of his main argument against believing in miracles.

DOES HUME'S WAY OF DEFINING "MIRACLE" LEAD HIM TO BEG THE QUESTION?

There is a tradition, going back at least to George Campbell's 1763 *Dissertation of Miracles*, of reading Hume as giving such a strict definition of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

"miracle" that he has in effect begged the question against miracles by defining them out of existence (Campbell 1763, 28).

Fosl (1999, 180 and 190 n. 45) discusses the charge of circularity in Hume's definition of "miracle" and provides further references. However, the textual evidence, as Fogelin points out, is undeniable: "Hume nowhere argues, either explicitly or implicitly, that we know that all reports of miracles are false because we know that no such experiences have ever occurred" (Fogelin 2003, 19).

Like Fogelin, I think it is a serious interpretive error to read Hume's argument as circular in this way, so I certainly do not want to be seen as instantiating the question-begging here as I show how Hume defines "miracle," as if the definition itself makes miracles impossible. Instead, we need to be clear about what Hume means by a miracle in order to delineate the scope of the discussion. With definitions of "miracle" different than Hume's, it will be possible to avoid Hume's conclusion. But Hume has excellent reasons for the definition he adopts, as I explain below.

Clearly, a violation of a law of nature will truly authenticate some specific prophecy or religious message if and only if the violation really does come about through the will of the deity who is supposed to be sponsoring the miracle. But it is a question distinct from any question about the occurrence of the supposed event itself whether or not a specific law violation (on the hypothesis that it did occur) can justifiably be attributed to a specific deity. This is a question fraught with its own skeptical difficulties, but it is one that Hume can sidestep since according to his argument we never have sufficient evidence for warranted belief that a violation of a law of nature has occurred in the first place.

Hume is careful to distinguish the merely wonderful or unusual—what he calls "marvels"—from the miraculous (see especially EHU 10.8 and 10.11). The birth of a baby, though wonderful, is clearly not a "miracle" (no matter how often people might speak that way), because events of that kind happen very often indeed (more than seven billion times in just the last century, only counting humans). Likewise events such as someone surviving a parachute accident, or a tossed coin landing on edge, though certainly unusual, must be classed as marvels rather than miracles since no laws of nature are violated.

Earman criticizes Hume's definition of "miracle" on the grounds that it is not consistent with Hume's contemporaries' usage. Earman says, for example, that "what matters is not how Hume classified examples [of miracles versus marvels] but how the major participants in the eighteenth-century miracles debate classified them" (2000, 11). This is an odd thing for Earman to say, however, since it is a common and well-accepted strategy in philosophy to refine the definition of a term in order to make it more precise, and then to draw philosophical conclusions on the basis of the redefinition. And Hume has excellent reasons for defining miracles

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

as he does, as is described below. In any case, Hume was certainly not alone among his contemporaries in defining miracles in this strict sense. William Fleetwood, for example, in *An Essay upon Miracles, in Two Discourses* (1701), defines a miracle as "*an extraordinary operation of God, against the known course, and settled laws of nature, appealing to the senses*."

For Hume's purposes, only miracles conceived as violations of laws of nature deserve the name, since only miracles in that sense could possibly provide an additional, independent kind of evidence for religious hypotheses. As Bitzer (1998, 178) points out, Fleetwood's definition of miracles, like Hume's, also carefully distinguishes miracles from the "merely" extraordinary. Events that happen conformably to the order of nature (that is, events that *can* happen, without there being a violation of a law, though they might rarely or never actually occur) provide no independent rational basis for religious belief precisely because they do not supersede the order of nature. Events occurring conformably to the order of nature, including marvels, can be invoked in design arguments. Hume's critique of design arguments occurs mainly in the *Dialogues concerning Natural Religion*, where he argues essentially that the claim that the world exhibits design is very much overstated, that the design hypothesis is no more likely than other speculative explanations of the observed facts about the world, and that design arguments are certainly not strong enough or detailed enough to support any particular conception of the deity.

CONTRARY MIRACLES

It was and perhaps still is commonly held by proponents of miracles that miracles can only be brought about by the one true God. Adherents of this view might say, Moses didn't part the Red Sea, God did it on Moses's signal. One could describe this as an analogous to special-case Cartesian occasionalism; the alternative is that Moses had real magical powers, which would be an undesirable conclusion for most theistic views. The miracles of Christ could either be the result of God's activity as in the case of the Mosaic miracles, or perhaps God delegated his power to his son who then performs magic of his own but with God's imprimatur.

If it is correct that miracles can only be wrought by the one true God, the fact that the purported revelations in the New Testament are attended with miracles would be solid grounds for thinking that the revelation is genuine, and thus for accepting Christianity as true—provided, of course, that the miracles actually occurred.

As Hume points out in the second part of his discussion of miracles (EHU 10.24), however, this way of trying to justify Christianity cannot succeed, even were one to accept the evidence for the occurrence of miracles. This is because there are many claims of miracles in the various

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

contrary religions, and if someone were to give credence to the miracles supporting one religion, they would be obliged by consistency to do the same for the miracles of other religions: "all the prodigies of different religions are to be regarded as contrary facts, and the evidence of these prodigies, whether weak or strong, as opposite to each other" (EHU 10.24). The testimonial evidence that supports miracle claims in other competing religions is all of the same type. The testimonial evidence about miracles, then, gives equally good grounds for accepting the revelations of two or more contrary religions, *but at most one of those religions can be correct*.

In a situation in which incompatible hypotheses have equally strong evidence, a good skeptic will suspend judgment, and believe in none of the miracles (and hence believe in none of the religions on the basis of those miracles). At most one set of religious miracles can have really occurred, and so almost all (if not all) of the many reports of miracles must be false. Since the evidential grounds are the same for each of the conflicting reports, it follows that we should accept none of them.

To put the argument more directly: Almost all miracle reports must be false, because miracle reports from contrary religions are mutually inconsistent with each other. If the miracle reports from one religion are true, those from all the other religions must be false. They assert inconsistent theological facts, so they cannot all be true together. There is no epistemic basis on which to distinguish the competing miracle reports; the evidence is of the same type and quality for all of them. Therefore, we should suspend belief about *every* miracle report. And, therefore, miracles cannot serve as a rational foundation for religious belief in any case. As Hume puts it, the claims of miracles in contrary religions are *mutually destructive* (compare EHU 10.8 and 10.24). A careful reasoner will have no grounds (from purported miracles at least) to prefer one religion over the others.

This is perhaps a reason to think that certain remarks in New Testament are actually self-undermining. The New Testament warns that many miracle claims are attempts to deceive: "4 And Jesus answered and said to them: 'Take heed that no one deceives you. 5 For many will come in My name, saying, *I am the Christ*, and will deceive many. . . . 24 For false christs and false prophets will rise and show great signs and wonders to deceive, if possible, even the elect'" (New King James Version, Matthew 24:4–24). If we accept the general warning that persuasive-seeming but nevertheless false testimony about miracles is common, how could we validate the Christian miracles? Hume's overall argument is that we cannot; if there is a basis for rational belief in Christianity, it must be elsewhere.

Wootten argues that since only monotheisms assert themselves to be exclusively true, only monotheisms are undermined by the contrary miracles arguments, and that Hume somehow missed this and assumed polytheisms were similarly undermined: "The argument from contrary

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

miracles is thus an argument against the different monotheistic religions which claim a monopoly of religious truth, but it is not an argument against polytheism, for a polytheist could admit that there was truth in the miracles of other religions" (Wootten 1990, 213). I disagree that the inference from contrary miracles does not apply to polytheisms. The laws of nature are universal, even if there are many supernatural beings. It would seem to follow that such laws cannot be overridden by "merely local" gods (tree sprites and the like). At the very least, there is only one true metaphysical description of reality itself; the liberality of polytheism cannot be literal. Instead, polytheists would have to say that we only have partial conceptions and understandings of various aspects of the divine, and that there is "truth" in competing religions only metaphorically or as an expression of our incomplete knowledge of the entire picture. And, of course, if this is all we can mean by truth in competing miracles, miracles are not an adequate foundation for rational religious belief. If polytheistic miracles are not supposed to be evidentiary, well, that means Hume's point is already made: They cannot then be a rational foundation for belief.

As an aside, note for later purposes that Hume directly connects this contrary miracles argument to legal standards of evidence: "This argument may seem overly subtile and refined; but it is not really different from the reasoning of a judge, who supposes, that the credit of two witnesses, maintaining a crime against one, is destroyed by the testimony of two others, who affirm him to have been two hundred leagues distant, at that the same time when the crime is said to have been committed" (EHU 10.24).

This subsidiary "contrary miracles" argument rests on accepting the testimony about miracles. But Hume's main point in "Of Miracles" is that we do not have adequate grounds to accept such testimony in the first place. The contrary miracles argument is, if you like, a fallback position: Hume defeats the credibility of miracle reports, but if someone thinks Hume has failed to establish the incredibility of miracle reports, then the contrary miracles argument still shows that miracle reports cannot serve as a rational foundation for religious belief.

OF MIRACLES AND LAWS OF NATURE

For Hume, it follows directly from the first definition of a miracle as a violation of a law of nature that no report of a miracle should be believed. This is because of Hume's epistemology of empirical facts and how this in turn leads him to characterize laws of nature, as the next few paragraphs show. I dive into related issues in more detail in chapter 3.

Hume divides all possible knowledge claims into two categories, *matters of fact* and *relations of ideas* (EHU 4.1). All our ideas, Hume says, have

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

their first origin in sensory or internal experience, to which he applies the general name *impressions*. This claim, known as Hume's "Copy Principle," appears in nearly identical form in both THN 1.1.1.7 and EHU 2.5. Through various modes of thought we can manipulate ideas that come directly from impressions to form new, compound ideas. Relations of ideas can provide us with certainty, effectively because they are analytic: "All bachelors are unmarried" is a relation of ideas, and it is certain. Matters of fact, in contrast, can never be certain. Instead, we know matters of fact to higher and lower degrees of probability, depending on the kind and strength of the evidence available. (See EHU sections 2–6.) A law of nature, according to Hume, is an empirical generalization that the human mind forms on the basis of an observed "constant conjunction" of event types—that is, two or more things always happening together—plus an expectation of the mind that future cases will resemble past cases. A law is thus not something we simply read off the world. The activity of the human mind, in particular the role of customs or habits of thought, Hume thinks, makes a crucial contribution. This means that for Hume laws of nature are epistemic rather than ontological categories. (There could be something lurking behind the appearances, such as real nomic necessity, but since we have no epistemic access to it, according to Hume's empiricist principles it cannot be part of a meaningful conception of a law of nature from our point of view.)

Peter Fosl (1999, 180) points out that Hume's argument against miracles has been criticized on the grounds that his conception of laws is vague or otherwise inadequate. Fosl answers this criticism in part by emphasizing that Hume's conception "conform[s] to the principal features of laws of nature developed by more recent philosophers of science." The things Hume discusses as laws or as grounded in laws are "true, non-analytic, universal generalizations, whose subject terms are unrestricted, that sustain counterfactual conditionals, and that may be used to formulate explanations and predictions of events in nature" (Fosl 1999, 180–81).

Even the best supported empirical generalizations, since they are constructed on the basis of inductive reasoning—which Hume famously shows to be non-demonstrative (see EHU 4, especially part 2)—cannot be known with certainty, but rather can only be known with some degree of probability. The appropriate degree of probability for a given empirical generalization is determined by the relevant available evidence.

There is a reasonable objection to Hume's position here that derives from other aspects of his epistemology. Hume holds that ultimately there is no rational foundation for inductive reasoning. If correct, this would seem to imply that it cannot be demonstrated that there is a correlation between the degree of probability of an empirical proposition and the degree of strength of the available evidence. Hume does not, however, embrace the radically skeptical implication here; instead, he appeals to a

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

distinction between *philosophical* contexts in which we recognize that knowledge has no rational foundation, and *practical* contexts in which it nevertheless does make sense to lay down methodological and epistemological rules in accordance with (what Hume takes to be) the basic principles of how the human mind works. On a cursory reading it can seem inconsistent for Hume to say that there is no rational foundation for inductive inferences and yet for him to make normative claims to the effect that we should believe inductive generalizations such as the law of gravity but that for evidential reasons we should not believe in miracles. A fuller reading, discussed in chapter 6, makes it clear that Hume can consistently make a normative epistemic distinction between these two kinds of knowledge claim despite his inductive skepticism. For now, let's continue with the main analysis of Hume's argument against miracles.

HUME AND THE EPISTEMOLOGY OF EMPIRICAL EVIDENCE

Remarking on knowledge of matters of fact, Hume writes:

> A wise man, therefore, proportions his belief to the evidence. In such conclusions as are founded on an infallible experience, he expects the event with the last degree of assurance, and regards his past experience as a full *proof* of the future existence of that event. In other cases, he proceeds with more caution: He weighs the opposite experiments: He considers which side is supported by the greater number of experiments: To that side he inclines, with doubt and hesitation; and when at last he fixes his judgment, the evidence exceeds not what we properly call *probability*. All probability, then, supposes an opposition of experiments and observations, where the one side is found to overbalance the other, and to produce a degree of evidence, proportioned to the superiority. . . . In all cases, we must balance the opposite experiments, where they are opposite, and deduct the smaller number from the greater, in order to know the exact force of the superior evidence. (EHU 10.4)

Several matters are worthy of comment here. It is a basic premise of Hume's skepticism (or of any reasonable epistemology) that belief should be proportioned to evidence. The Bayesian epistemology Earman advocates is similarly concerned with determining degree of belief in light of the evidence available to an epistemic agent in a given context. Only someone steeped in the notion that degrees of belief can have precise numerical values, however, would read the final lines of the passage quoted here as suggesting that Hume thought that determining the degree of probability of some proposition was a matter of literally *subtracting* the number of contrary observations from the number of positive ones. As I argue below, Hume definitely does not think that degrees of belief can be treated as having precise numerical values.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

A major difference between Hume and the Bayesians is that, for Hume, degree of belief is determined by the purely psychological characteristic "force and vivacity" and that for Hume probable reasoning is founded on the associationist principles of resemblance, contiguity, and (most especially) cause and effect. This is an aspect of Hume's idea-epistemology that many commentators have taken to be flawed. It is worth noting, however, that Collier (2005) demonstrates that contemporary cognitive science and learning theory have come to see that Hume's associationist theory of probabilistic reasoning is borne out by psychological experiments. Likewise, Jerry Fodor (2003) thinks that Hume's cognitive psychology is largely correct and that it matches up rather well with what the field of cognitive science tells us today. Collier argues that, "Hume's theory of probabilistic inference is neither misguided nor inadequate; quite the contrary, it stands at the leading edge of our contemporary science of the mind" (Collier 2005, 21). His evidence comes from experiments on human subjects and simulations based on computational models of neural networks. That said, I think that Hume's overall argument against miracles would survive even if we did need to replace Hume's idea-epistemology with something else, but the details of that are beyond the scope of this book.

An important consideration for Hume is that the *quality* of the evidential force of competing observations or experiments (or of testimony about them) can vary; as he says, the first step is to *weigh* the opposite experiments (EHU 10.4). So for Hume it is not simply a matter of comparing the number of competing observations on each side. And when Hume says we should "deduct" competing experiments and observations from one another to get the "exact" force of the superior evidence, he is using language that is doubly unfortunate from the point of view of our own age, since he does not mean that the precise numerical probability is obtained thereby, but merely that a careful and properly nuanced judgment about what should be believed in a given case is achieved. Comparing pieces of evidence is a matter of judgment for Hume, not merely a matter of calculation. In this vein, there is an old legal saying mentioned by Leibniz in a 1698 letter to Wagner: "Reasons are not to be counted, but weighed" (as quoted in Franklin 2001, 365).

In Hume's epistemology, mathematical calculations are *relations of ideas*, things that can be known with absolute certainty, whereas empirical *matters of fact* are not the sorts of things that can be known with certainty. It would thus have seemed to Hume to be a kind of category mistake to discuss degrees of belief regarding matters of fact in mathematical terms. This is one reason that Hume does not discuss evidential probability numerically even though some of his contemporaries, such as Richard Price, were beginning to do so. In treating evidential probability in a deliberately non-mathematical way, as I show in chapter 4, Hume is by no means alone.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

For Hume, we can at best achieve "moral certainty" or "moral evidence" with regard to empirical propositions, which is a degree of assurance sufficient for action and belief but short of perfect certainty (EHU 4.18). Jean Gerson, chancellor of the university at Paris around 1400, seems to have been the first to use the phrase "'moral certainty' (*certitudo moralis*) to mean a very high but not complete degree of persuasion" (Franklin 2001, 69). To borrow Franklin's fine phrase: "Just as a suspected criminal is not a kind of criminal, so moral certainty is not a kind of certainty" (Franklin 2001, 70). The concept of "moral certainty" is used in precisely this way very widely in early modern philosophy, notably by Descartes (especially at the end of *The Principles*) and Hume (who often speaks of "moral evidence").

As Hume notes (EHU 10.3–4 and EHU 6, n. 10), degrees of assurance with regard to empirical propositions can vary along a scale of probabilities from "full proof" down through various lesser degrees of probability. It is therefore vital to recognize that when Hume writes of the possibility of having evidence regarding matters of fact that amounts to a "full proof," he is not talking about *demonstration*. Demonstration is achievable only with regard to relations of ideas. "Proof," in Hume's vocabulary, is a probabilistic category:

> Mr. LOCKE divides all arguments into demonstrative and probable. In this view, we must say, that it is only probable all men must die, or that the sun will rise to-morrow. But to conform our language more to common use, we ought to divide arguments into *demonstrations*, *proofs*, and *probabilities*. By *proofs* meaning such arguments from experience as leave no room for doubt or opposition. (EHU 6, n. 10)

Hume says very similar things in the *Treatise*:

> One wou'd appear ridiculous, who wou'd say, that 'tis only probable that the sun will rise to-morrow, or that all men must dye; tho' 'tis plain we have no farther assurance of these facts, than what experience affords us. For this reason, 'twou'd perhaps be more convenient, in order at once to preserve the common signification of words, and mark the several degrees of evidence, to distinguish human reason into three kinds, viz. *that from knowledge, from proofs, and from probabilities*. By knowledge, I mean the assurance arising from the comparison of ideas. By proofs, those arguments, which are deriv'd from the relation of cause and effect, and which are entirely free from doubt and uncertainty. By probability, that evidence, which is still attended with uncertainty. (THN 1.3.11.2)

So, when Hume writes of evidence that amounts to a "full proof," he is not talking about *demonstration*. Demonstration can only be achieved with regard to relations of ideas. Proof for Hume is a probabilistic category that applies only to empirical matters of fact. Many critics of the argument against miracles have missed this point. Some commentators, in-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

cluding Earman, mistakenly suppose that when Hume concludes that the exceptionless experience on which laws of nature are based amounts to a "proof" against miracles, he means that the probability of the occurrence of a miracle is "flatly zero" (see chapter 5). This is, however, to unjustly accuse Hume of committing a category mistake according to Hume's own epistemology. For Hume, whether or not a miracle has occurred is a matter of fact, so its non-occurrence cannot be logically necessary. As Hume himself says prominently, "The contrary of every matter of fact is still possible; because it can never imply a contradiction" (EHU 4.2).

The word "proof" is today often used, especially among philosophers and mathematicians, to refer to *demonstration*, as in a proof in logic or mathematics. This is not, however, the only common use of the word: "proof beyond a reasonable doubt" in criminal law, for example, clearly implies the possibility of lesser and greater degrees *of proof*, and also implies that what has been proven is possibly (though not probably) false.

CONCLUSION OF CHAPTER 1

This chapter has described Hume's definition of a "miracle" as violation of a law of nature, as distinct from a marvel or merely unusual event, and it has noted that this definition is not susceptible to the charge of circularity. This chapter has also described the basic structure of the argument against miracles as a comparison between the extensive and perfectly regular experience that leads us to acquire knowledge of laws of nature, versus the fallible testimony with regard to supposed violations of those laws. With careful attention to Hume's distinction between demonstrations, proofs, and probabilities, we can understand Hume's argument that the evidence for the occurrence of a miracle never rises to the level of probability, much less to the level of proof. At the same time, the available evidence provides a complete proof of the law of nature in question. Therefore, we never have sufficient evidence from testimony to believe that a miracle has occurred: The balance of evidence is always in favor of the law of nature. Even if this argument fails, Hume can fall back to his subsidiary argument that the evidence for miracles from completing religions cancel each other out, so that even in that case miracles still cannot be a foundation for rational belief in a religious hypothesis. The next chapter examines in further detail Hume's reasoning about the role of testimony in attempts to establish miracles.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

TWO
Testimony

LOCKE AND HUME ON TESTIMONY

In 1846, Simon Greenleaf published a tract in which he used the standards of legal evidence and proof to interrogate (in both the legal and the post-modern senses of the word) the evidence for Christian religious claims. The work is not usually considered a philosophical or literary success, but Greenleaf *does* accurately summarize how the law views the credibility of witnesses: "The credit due to the testimony of witnesses depends upon, firstly, their honesty; secondly, their ability; thirdly, their number and the consistency of their testimony; fourthly, the conformity of their testimony with experience; and fifthly, the coincidence of their testimony with collateral circumstances" (Greenleaf 1984 [1846], 28).

This chapter delves into details of particular aspects of Hume's argument against miracles: the epistemology of testimony in general; Hume's understanding of laws of nature; and then testimony about miracles (violations of laws of nature) in particular.

Immediately following the passage quoted in chapter 1 about proportioning belief to evidence (EHU 10.4), Hume makes the point that in ordinary life nothing is more important to the process of judging the relative weight of evidence than is receiving and evaluating the testimony of others (EHU 10.5). This leads Hume to his first major claim regarding miracles, namely that no testimony about a miracle is sufficient to "establish"—that is, no miracle is sufficient to provide epistemic warrant adequate for rational belief in the fact or to assent to the claim—that a miracle has actually occurred (EHU 10.12-13). Hume's reasoning here is essentially just that our evidence for the laws of nature will always be stronger than the evidence supplied by any testimony about an instance in which a law was supposedly violated.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Hume's account of the epistemology of testimony resembles John Locke's, though the two differ in some important details and conclusions. In "Of Probability," chapter 15 of Book IV of the *Essay concerning Human Understanding* (1690), Locke argues that the trust that should be put in testimony depends on several factors, chief among which is *the conformity of the testimony with the rest of our experience*. Locke makes this clear with the example of a man walking on a frozen pond in December in England. To someone from England, testimony about this event is in conformity with her own knowledge and experience, and the testimony is therefore to be judged credible by her. To someone from the tropics, the same testimony is *and should be* much less credible (because belief is to be proportioned to the available evidence, which includes the prior experience through which we evaluate testimony). "[T]o a man . . . [who] has never heard of anything like it, the most untainted credit of a witness will scarce be able to find belief" (Locke 1690, 4.15.5.2, 656). Thus, the epistemic context makes a difference to the weight that should be attached to various kinds of evidence, including third party testimony.

For Locke the result is a hierarchy of degrees of probability concerning matters of fact. Those cases in which one should have the highest degree of confidence are those where the testimony of fair witnesses is consonant with our own constant experience and the experience of every person in every age (so far as we can tell). In such cases we have what Locke calls *assurance*, and we act *as if* the thing were certain, even though no matter of fact is truly certain. What Locke calls "assurance" is thus equivalent to what Hume calls "full proof." We have a degree of probability Locke calls *confidence* in those empirical propositions that, in our own experience and the experience of others, happen *for the most part* in a given way. Our assent is *unavoidable* with regard to events that might happen one way or another ("indifferently") when there is no reason to doubt the witnesses. And so on. Hume concurs that "in our reasonings concerning matters of fact, there are all imaginable degrees of assurance, from the highest certainty to the lowest species of moral evidence" (EHU 10.3). But as Locke says,

> The difficulty is, when testimonies contradict common experience, and the reports of history and witnesses clash with the ordinary course of nature, or with one another; there it is, where diligence, attention, and exactness is required, to form a right judgment, and to proportion the assent to the different evidence and probability of the thing: which rises and falls, according as those two foundations of credibility, *viz.* common observation in like cases, and particular testimonies in that particular instance, favour or contradict it. (Locke 1690, 4.16.9, 663)

Using as an example the principle of English common law that admits a certified copy of a document as legal evidence, but never a copy of a copy no matter how well certified, Locke also lays down two principles: that

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

"any testimony, the further off it is from the original truth, the less force and proof it has" (Locke 1690, 4.16.10, 663–4), and that "no probability can arise higher than its first original" (Locke 1690, 4.16.11, 664). Contrary to a recent claim that "Locke has no account of the evidential force of testimony at all" (George 2016, 35), then, we see that Locke indeed has a robust account of the epistemology of testimony.

The connection between Locke's general account of the epistemology of testimony and Hume's argument against miracles is straightforward. For Hume, our experience of the constant conjunction of two types of events is the foundation for our expectation that the world will continue to be regular in that respect. Since the experience on which a given law of nature is founded is exceptionless, a report of a singular violation of a law of nature is not to be believed.

As Hume writes: "The very same principle of experience, which gives us a certain degree of assurance in the testimony of witnesses, gives us also, in this case, another degree of assurance against the fact, which they endeavour to establish; from which contradiction there necessarily arises a counterpoise, and mutual destruction of belief and authority" (EHU 10.8). This "counterpoise, and mutual destruction" is the basic principle: Hume fills in the details by showing what "degree of assurance" we have on either side in the case of miracles, namely a proof from uniform experience in favor of laws of nature, against some lesser probability from the testimony to a miracle. The counterpoise principle is something Hume simply reads off of our ordinary behavior in evaluating testimony—whether in ordinary life or in the law. The strength of the evidence for laws of nature comes from Hume's interpretation of how we arrive at laws. The degree of assurance of testimony to marvelous and miraculous events is again something we derive simply from observation of cognitive behavior: No wise person believes reports of fairies and UFOs, and it is only religious prejudice or enthusiasm that leads people to treat reports of miracles differently.

As an aside, it is worth pausing to note that we have the same total evidence for religious miracles as we do for the miracles of Santa Claus, too. Santa miracles? Well, yes: flying reindeer, knowing whether every child has been naughty or nice, fitting down every chimney despite his girth, getting to every house in one night, ingesting nearly infinite amounts of milk and cookies, and so on. How do we know about Santa's omniscience, omnipresence, benevolence, and great works? Someone said so, wrote it down, sang songs about it; there is a long tradition about it. At night when they are half asleep, over-enthusiastic children sometimes convince themselves that they heard the hooves or bells, or that they saw Santa himself. Parents even sometimes deliberately promote such delusions, through everything from false reassurances to planting ideas to faking Santa appearances. Just about every adult would agree, however, that the testimony about Santa we heard as children was wrong

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

through misperception, mistransmission, and (mostly) deception. We do this to our own children! Hume seems to have been right about the knavery and folly of humans, and right to think that that we should look for alternative, ordinary explanations when we are told fantastical stories.

Perversely, in the case of Christian miracles Locke denies the seemingly obvious consequence of his own general position on testimony. Locke claims, instead, that when the outcome of some event that is contrary to nature is consistent with furthering God's ends, testimony about that miracle should engender a *greater* degree of belief in the occurrence of the event the *farther* the event is from the ordinary course of nature (Locke 1690, 4.16.13, 667, and Locke's *A Discourse of Miracles*, 1706). But since Locke must assume that he knows what God's ends are and what sorts of acts are worthy of God, from Hume's point of view Locke would be begging the question with regard to the evidential value of reports of miracles. Hume thus corrects Locke's position and makes the empiricist account of miracles consistent with the empiricist account of the credibility of testimony found in Locke.

In *Ideas, Evidence & Method: Hume's Skepticism & Naturalism concerning Knowledge & Causation*, Graciela De Pierris notes that a significant difference between Locke and Hume on probability, even between Lockean assurance and Humean proof, is that Locke is interested mainly in the probability of particular facts, not inductive generalizations. Locke still retains the ideal of demonstrative reason as the source of knowledge, and thus holds that knowledge of laws of nature (Hume's prime example of matters of fact known to the level of moral certainty through inductive reasoning) is not for Locke acquired inductively or probabilistically (De Pierris 2015, 193). Granting this caveat, Hume's account of probability is clearly similar to Locke's. De Pierris's analysis suggests the point that the reason Locke did not arrive at Hume's position on miracles is that Locke's account of laws of nature—as deriving from demonstrative knowledge of the qualities and powers of bodies—makes such knowledge immune to disconfirmation by empirical considerations.

Coleman (1988, 344, n. 5) and Fogelin (2003, 16) also mention the connection between Hume's and Locke's accounts of probability, testimony, and miracles. Fogelin (2003, 90, n. 4) refers as well to Middleton's (1749) *Free Inquiry* and to Arnauld and Nicole's (1662) *Logic or the Art of Thinking* as embodying similar principles as Locke and Hume for the evaluation of testimony regarding miracles. This convergence is not merely the result of such new ideas being "in the air" in the modern period. Rather, as I show later, each of these authors is picking up on an ancient legal tradition of thinking about evidential probability, one that also informed the development of the mathematical theory of probability. Thus, we do not need to trace specific influence between all these authors—the tradition in which they all participate is the common cause of their similar sentiments. I discuss this tradition in detail in chapter 4.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

LOGIC OR THE ART OF THINKING

Wooten's (1990) account of the sources of Hume's thinking in "Of Miracles" gives a prominent place to the famous Port-Royal Logic by Antoine Arnauld and Pierre Nicole (1996 [1662]), formally titled *Logic or the Art of Thinking*. Wooten remarks, "the paradigmatic treatment from this point of view [that the question of miracles could be narrowed down to a question of the credibility of testimony] was still Arnauld and Nicole . . . [in which] the question of miracles had become not merely a question of the credibility of testimony, but one whose analysis was seen to involve balancing the inherent improbability of the miraculous event against the apparent reliability of human testimony" (196). It is instructive to consider in detail what Arnauld and Nicole say about testimony, probable reasoning, and miracles.

In part 4, chapter 13 of *Logic or The Art of Thinking*, Arnauld and Nicole (1996 [1662]) discuss reasoning about events that depend on human faith, that is, "judgements we make about what takes place every day in human affairs" (262). These judgments concerning the truth or falsity of propositions about things that have happened in the past, or which may happen in the future, are the objects of our belief and anticipation. These are "matters of faith" not in a religious sense but in the sense that we have uncertain knowledge about them and yet must or do believe one way or another.

Since human events are by their nature contingent, Arnauld and Nicole say, "we will make a thousand fallacious inferences about them" if we attempt to apply to them the rules that are for reasoning about the natures and essences of things, which are necessary. This is, essentially, Aristotle's injunction to expect the degree of certainty appropriate to the subject matter: "[I]t is the mark of an educated man to look for precision in each class of things just so far as the nature of the subject admits: it is evidently equally foolish to accept probable reasoning from a mathematician and to demand from a rhetorician demonstrative proofs" (Aristotle 1984, *Nicomachian Ethics*, 1094b24–27)

Arnauld and Nicole go on to argue that it would be absurd to believe a proposition (the example given is that the king of China has converted to Christianity) simply because it is not impossible—the contrary of that claim is not impossible, too, and hence the evidence of non-impossibility does not decide the question. So, they propose the following maxim as the way to decide whether to believe contingent propositions:

> In order to decide the truth about an event and to determine whether or not to believe in it, we must not consider it nakedly in itself, as we would a proposition of geometry. But we must pay attention to all the accompanying circumstances, internal as well as external. I call those circumstances internal that belong to the fact itself, and those external that concern the persons whose testimony leads us to believe in it.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> Given this attention, if all the circumstances are such that it never or only rarely happens that similar circumstances are consistent with the falsity of the belief, the mind is naturally led to think that it is true. Moreover, it is right to do so, above all in the conduct of life, which does not require greater certainty than moral certainty, and which even ought to be satisfied in many cases with the greatest probability. (Arnauld and Nicole 1996 [1662], 264)

Note that, here at least, the Port-Royal Logic takes a position very similar to Hume's regarding the respective epistemic statuses possible for relations of ideas and matters of fact. This includes an apparently identical account of moral certainty as being the maximum degree of certainty possible in life and "the greatest probability" normally being sufficient for practical belief and practical action. (The concepts of internal and external circumstances, by the way, come from the Roman legal tradition.) And, like Hume, Arnauld and Nicole hold that a balance of probabilities based on considering all the relevant information is the way to decide one's degree of assent to a contingent proposition. They continue: "But if, on the contrary, these circumstances are such that they are often consistent with the falsity of the belief, reason would require either that we remain in suspense, or that we view as false whatever we are told when its truth does not look likely, even if it does not look completely impossible" (Arnauld and Nicole 1996 [1662], 264).

Up to this point, I see no serious problems with this account of evidence and testimony, but in their next move Arnauld and Nicole seem to me to go off the rails. The case seems similar in some respects to the way that Locke presents a coherent account of reasoning about empirical probabilities but then contradicts his own position in order to make believing in miracles appear rational. The apparently solid general principle expressed by Arnauld and Nicole regarding considering the balance of probabilities within the context of the total evidence receives this caveat, which later gets used to dubious effect:

> There is, however, an exception to this rule, when we ought to be satisfied with possibility and likelihood. This is when a fact that is otherwise sufficiently confirmed is beset by difficulties and apparent contradictions with other stories. In that case it is enough if the solutions brought to the contradictions are possible and likely. It is acting against reason to require positive evidence of them, because when the fact is sufficiently proved in itself, it is not right to require similar proof of all the circumstances. Otherwise we could doubt a thousand well-established histories that can be reconciled with other histories that are no less certain only by conjectures that are impossible to prove positively. (Arnauld and Nicole 1996 [1662], 265)

Their way of speaking here may be a bit hard to follow. The main idea seems to be this: When conflicting testimony casts doubt on an otherwise probable claim, we can treat the apparent contradiction as sufficiently

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

resolved when we can provide a resolution that is itself merely "possible and likely." Those resolutions themselves don't require more than possibility, since otherwise we would end up having to doubt too much. For example, if we have a well-established history (the Sandy Hook mass shooting, say) that is contradicted by one piece of evidence (Alex Jones's assertions), we could simply say that that the contradictory piece of evidence is wrong.

"If we fail to observe [the rule just explained] we are in danger of falling into the dangerous extremes of credulity and skepticism" (Arnauld and Nicole 1996 [1662], 265). Just as we recognize the foolishness of someone who believes every story describing a purported miracle, Arnauld and Nicole hold it to be a mistake to have a principle of doubting all miracles "without having other reasons than that frequently miracles were reported that were found not to be authentic, and that there is no more reason to believe in some than in others" (1996 [1662], 266).

But then, in part 4, chapter 14, Arnauld and Nicole undertake the "Application of the proceeding rule to beliefs about miracles." It seems clear in retrospect that they have designed their principles in chapter 13 precisely to make it possible for them to assert the rationality of belief in the miraculous in chapter 14. The problem is that the principle works when we are talking about well-established facts—then, "merely contrary" claims do seem to be simply dismissible—but it does not work when we are talking about events that are *not* well-established. Or, to put the point another way, my position is that while it is not wrong to defend well-established laws and regularities from "merely contrary" claims, it *is* incorrect to say that a *miracle claim* should be protected from contrary claims derived from the constancy of nature or the (constant) inconstancy of human nature. Hume, I think correctly, makes it that the miracle claim is the "merely contrary" claim against the well-established regularities of nature.

Arnauld and Nicole tell a story of miracles that were purported to have occurred in Milan as reported by St. Augustine, who says he was in the city at the time. They conclude, "So the only remaining basis for skepticism would be to doubt the testimony of St. Augustine, and to suppose that he altered the truth to legitimize the Christian religion in the minds of pagans. Now no one can say this with the slightest plausibility" (1996 [1662], 269).

The world suffers here from there not being a well-established sarcasm font; for, surely, if Hume or Montaigne had written this final line, interpreters would have said it was not meant to be taken literally. As it is, we must remain in doubt as to whether Arnauld and Nicole can really be serious. One suspects that they are, given their religious commitments, but even so it seems highly doubtful that they are expressing a genuinely rational, evidence-based position, at least not one that could be rationally accepted by someone who is not already antecedently committed to the

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

honesty of Augustine and the truth of Christianity. The possibility that a person could lie or bend the truth to draw people to a cause that is in their own interest is all too common, or so we would judge from our own historical position. But propaganda can hardly be only a recent phenomenon. Hume, at least, holds that self-interested testimony casts some doubt on the probability of the facts testified, especially when we know of similar cases where the testimony turned out to be corrupt.

Hume might also add here that in addition to what Arnauld and Nicole call the "external circumstances" about the testimony being self-interested in the case of the miracles attested by Augustine, the "internal circumstance"—regarding the supposed event itself—is simply implausible because it contradicts the rest of human experience. To put it another way, the purportedly miraculous event in question is not one that should count as "well-established" or "highly warranted" epistemically even if there is a long tradition of people *accepting* it; doubts about that event are not "mere doubts" of the sort that can be dismissed in the kinds of cases Arnauld and Nicole's principle was initially supposed to cover. Again, this is not to say that Hume therefore judges such implausible events to be impossible but rather that he judges that claims seeking to establish the occurrence of such implausibilities must have an impressive degree of evidence, a degree clearly not present in this particular example. As Wooten puts it, Hume's argument against miracles seems designed to turn Arnauld and Nicole's position on its head: "where Arnauld argued that honest testimony could be accepted even for the most improbable events, Hume argued that certain events are so improbable that no testimony could be strong enough to make them credible" (1990, 197). Wooten points out that Locke's treatment of the testimony of miracles derives from the Port Royal Logic, too, and thus Locke's position simultaneously became a target of Hume.

In partial defense of the position taken by Arnauld and Nicole, we could note that rhetoric and dialectic in the seventeenth century taught that "the best and indeed often the only way to establish the truth of testimony was *via* the authority of the witness" (Serjeantson 1999, 205). Arnauld and Nicole seem to have been just on the edge of the historical turn that led to the overthrow of the idea that the testifier's virtue/reputation was important to the trustworthiness of the things to which they testified. Hume, one could say, effectively argued that no one's reputation—which after all, can only be known through a regularity of experience between their claims and the truth—was strong enough to back a claim that flies in the face of a perfect, otherwise-universal regularity of experience such as we have in the case of the laws of nature. We may add here that, among sixteenth century writers on the probative force of testimony, for example, "No human testimony is ever considered more than probable" (Serjeantson 1999, 206); in the developing natural philosophy of the late Renaissance and early modern periods, a strong theme was the

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

rejection of the notion of authority and hence a marked diminishment in the epistemic force of testimony with regard to questions of natural fact (Serjeantson 1999, 214). If we are not to accept testimony regarding ordinary natural historical facts (but should instead look to observation, experiment, and reason in order to acquire knowledge of them), then of course we should reject testimony that a law of nature has been violated.

TESTIMONY IN THE *TREATISE*

This section examines some of Hume's comments on the evidence of testimony in the *Treatise*, with the goal of illuminating his general epistemological position and supplementing the account of testimony regarding miracles from the *Enquiry*.

As Wootten points out,

> probability lay at the heart of the *Treatise*, occupying the whole of part III of Book One [*sic*]. There Hume discussed not only his new account of causation, but also the traditional topics of probability theory: chance, testimony, historical evidence. Missing only, in the published version, was a discussion of miracles, a topic which any philosophically educated person would have expected to see discussed in the same context. A clear indication that 'Of miracles' represents the missing discussion of miracles from the *Treatise* is the fact that its core arguments are presented in terms of probability theory. (Wootten 1990, 199)

This is correct except, as I argue elsewhere here, Wootten should have more accurately simply said "probability" instead of "probability theory" since the latter phrase implies (and Wootten intended, though I think incorrectly) the mathematical theory of probability.

Hume explains that we have two systems through which we attach the title "reality" to ideas and impressions. The first is the system of impressions and ideas of memory: Things that we currently perceive and things that we remember perceiving, we think of as real. Through the relation of cause and effect we also attach the title "reality" to ideas we arrive at through judgment. (THN 1.3.9.3.) Cause and effect, note, is founded on a custom or habit of the mind, and is "necessary" only in the sense of the expectation being *unavoidable for us*.

> 'Tis this latter principle [namely, *judgment*], which peoples the world, and brings us acquainted with such existences, as by their removal in time and place, lie beyond the reach of the senses and memory. By means of it I paint the universe in my imagination, and fix my attention on any part of it I please. I form an idea of *Rome*, which I neither see nor remember; but which is connected with such impressions as I remember to have receiv'd from the conversation and books of travellers and historians. (THN 1.3.9.4)

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Just a bit later Hume emphasizes the point that "the relation of cause and effect is requisite to perswade us of any real existence" (THN 1.3.9.6). From this foundation Hume goes on to explain the epistemic structure of testimony, and its persuasive (though not always rational) force.

Hume writes that "No weakness of human nature is more universal and conspicuous than what we commonly call CREDULITY, or a too easy faith in the testimony of others; and this weakness is also very naturally accounted for from the influence of resemblance." He explains:

> When we receive any matter of fact upon human testimony, our faith arises from the very same origin as our inferences from causes to effects, and from effects to causes; nor is there any thing but our experience of the governing principles of human nature, which can give us any assurance of the veracity of men. But tho' experience be the true standard of this, as well as of all other judgments, we seldom regulate ourselves entirely by it; but have a remarkable propensity to believe whatever is reported, even concerning apparitions, enchantments, and prodigies, however contrary to daily experience and observation. (THN 1.3.9.12)

Several important things are mentioned here: that experience is the proper guide for evaluating judgments; that we sometimes make judgments that experience does not warrant; that we have a natural tendency to believe whatever is reported in testimony even when we should not; and that we can correct this kind of mistake by careful attention. The passage continues with an explanation of why we tend to accept the evidence of testimony:

> The words or discourses of others have an intimate connexion with certain ideas in their mind; and these ideas have also a connexion with the facts or objects, which they represent. This latter connexion is generally much over-rated, and commands our assent beyond what experience will justify; which can proceed from nothing beside the resemblance betwixt the ideas and the facts. Other effects only point out their causes in an oblique manner; but the testimony of men does it directly, and is to be consider'd as an image as well as an effect. No wonder, therefore, we are so rash in drawing our inferences from it, and are less guided by experience in our judgments concerning it, than in those upon any other subject. (THN 1.3.9.12)

This is an exceedingly interesting claim. Hume is saying that what testimony ought to convince us of are just *facts about the contents of the reporter's mind*: Testimonial reports ought to make us believe that the reporter has particular ideas. The reporter *believes* that those ideas are linked with facts in the world (via impressions); we know from our own experience that ideas are ultimately derived from impressions; and so, we generally accept the other person's testimony as describing facts in the world. In uncritical moments, however, we initially neglect the fact that ideas

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

sometimes have other sources besides direct sensation: These include judgment and memory (which are fallible), imagination (which forms compound ideas that do not necessarily correspond to any existent things), and so on. The connection between reported ideas and facts is "much over-rated" in the sense that we have a strong psychological tendency to assent to it more than we should—that is, more than experience would justify. This means that the link between testimony, ideas, and facts in the world is less solid than we generally take it to be when we are receiving testimony. Furthermore, Hume is able to account for why we make this kind of mistake in judgment (that is, the mistake of credulity): The relation of the resemblance of ideas draws us beyond what experience justifies. The upshot is that, being aware of this tendency to ascribe too much reliability to the testimony of others, reason needs to correct the unconscious operations of the mind to form a balanced judgment.

This account is all the more understandable when we remember that for Hume degree of belief is just the degree of vivacity of the idea, and that there are several (not purely rational) mechanisms for producing vivacity. One important such mechanism is the relation of cause and effect. Likewise, resemblance and contiguity are relations that can increase the degree of vivacity of a belief, sometimes in ways that are unwarranted. Other mechanisms include things such as repetition. Even liars eventually believe their own lies—that is, they have ideas with a high degree vivacity—if they repeat the lies often enough (see THN 1.3.9.19). Hume also notes that when high emotions accompany ideas, this will increase their vivacity (see THN 1.3.9.15). Clearly, purported cases of religious miracles are situations in which believers' emotions are heightened. This in itself explains the high degree of vivacity of their ideas about the purported miracle, and the enthusiasm with which they give their testimony about the event. It also explains why their hearers tend to have a higher degree of belief in the propositions expressed in the testimony than would be justified based only on careful reasoning from the evidence. Hume is not quite so explicit on these topics in EHU, but the overall picture is essentially the same.

DOES INVOKING THE PRINCIPLE OF THE UNIFORMITY OF NATURE MAKE HUME'S ARGUMENT CIRCULAR?

Hume's analysis of miracle reports shows that there is far more evidence from experience that a given law is correct than there could be evidence from testimony that a violation of that law has occurred. Given the extensive uniform experience that backs up a law, it is very unlikely that the law will turn out to be wrong. At least, it will be unlikely for the law to be wrong provided that the Principle of the Uniformity of Nature continues to hold. The Principle of the Uniformity of Nature says that "future cases

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

will resemble past cases." It is the principle on which all inductive reasoning depends. Besides our psychological predilection to think in terms of the Principle of the Uniformity of Nature, it is also a maxim that is indispensable for the investigation of the world, in the sense that if we do not act as if the Principle of the Uniformity of Nature is true, no scientific or even ordinary empirical reasoning (for example, "the floor will continue to support my weight") would ever be possible at all.

One of Hume's key skeptical points is that there is no rational foundation for using the Principle of the Uniformity of Nature—psychologically we simply cannot avoid using it, and we have no reason to expect it to be true even if we cannot investigate the world without assuming that it is true. I will have more to say about this in chapter 6, but for now put this skeptical point aside (Hume has a good answer to it) and let us consider a different potential problem with the invocation of the Principle of the Uniformity of Nature.

The potential problem in question is that one might see circularity here in Hume's case against miracles. If, in order to investigate the world, we must *assume* uniformity, do we not thereby rule out (a priori, as it were) the very possibility of future events that are not in conformity with past experience, that is, where nature turns out not to be uniform?

I argue that this is not actually a worry for Hume's position. The maxim in question, the Principle of the Uniformity of Nature, is merely a regulative ideal for investigation of the world, rather than a claim regarding a supposed truth about the world. The maxim is defeasible and, more importantly, re-conceptions of what things count as true regularities are perfectly possible given the appropriate evidence. Thus, exceptions to established laws, or even new laws that contradict previous experience, *are* discoverable, provided that the right sorts of new evidence become available. Hume does not exclude this possibility; he just thinks it would be an extremely unusual and unlikely circumstance.

Note, too, that we must rely on the Principle of the Uniformity of Nature in various ways when we assess the credibility of testimony, too. Without it, we cannot make inferences from past experience regarding testimony of a certain kind to the probative value of current similar testimony. If we were to give up the Principle of the Uniformity of Nature in the case of laws of nature, consistency would force us to give it up in the case of testimony as well. Then, no inferences about matters of fact would ever be possible. In short, Hume does not intend relying on the Principle of the Uniformity of Nature to rule out miracles a priori; and anyway, all empirical enquiry would be impossible without the Principle of the Uniformity of Nature.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

HOW CAN WELL-ESTABLISHED LAWS BE OVERTURNED BY NEW EVIDENCE?

Part of what is at issue in Hume's examination of miracles is whether testimony is likely in practice ever to be able to provide sufficient warrant to overthrow a well-established law. With his negative answer Hume is in effect specifying the conditions under which it would be empirically justified to put aside a well-established law. The falsity of the testimony would have to be a "greater miracle" (EHU 10.13)—that is, would have to be more unlikely—than the falsity of the law. In this respect there is an interesting similarity between Hume's case against miracles and the fourth of Newton's "Rules for the Study of Natural Philosophy," in which Newton specifies the conditions under which evidence would be sufficient to permit overthrowing an already well-established law, such as his own law of Universal Gravitation. Similarly, when Newton says that he "feigns no hypotheses" (Newton 1999, 943), he means that he does not merely speculate about physical causes. Newtonian laws of nature are not mere hypotheses but instead are empirical generalizations generated in accordance with a set of strict methodological rules. (For more on Newton's scientific method, a good place to start is Cohen and Smith [2002], especially the articles by Harper and by Smith.)

Newton's Fourth Rule for the Study of Natural Philosophy reads: "*In experimental philosophy, propositions gathered from phenomena by induction should be considered either exactly or very nearly true notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact or liable to exceptions*. This rule should be followed so that arguments based on induction may not be nullified by hypotheses" (Newton 1999, 796; italics in original).

In Newtonian terms, Hume might have said that a miracle report is a "mere hypothesis"—a hypothesis that is contrary to a thoroughly established law, and whose evidential warrant is not sufficient for that hypothesis to be taken seriously as a rival to the established law. However, this apparent connection between Newton and Hume could result merely from coincidence rather than influence, since there is some evidence that Hume was never exposed to the third edition of Newton's *Principia*, in which the fourth rule makes its first appearance (Schliesser 2008). Another possibility is that what Newton says about mere hypotheses in the *Opticks*, which we know Hume did encounter, is similar enough to have influenced Hume in this way. Or, both authors could have been influenced by mutual historical antecedents. Whatever the explanation, the two ultimately have similar views about radical proposals that contradict well-established empirical generalizations: Do not accept the reality of exceptions to laws unless the evidence is very strong.

Some brief examples to show other connections between Newton and Hume are appropriate here. No science, says Hume, can go beyond expe-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

rience (see Hume's Introduction to the *Treatise*, THN I.10). At THN 1.1.4.6, Hume mentions Newton's principle of attraction and remarks that "nothing is more requisite" of a true philosopher than to avoid empty speculation about "true causes." Newton makes a point of saying that while he has discovered the laws describing gravitational interaction, he has made no assertions about the causes or physical mechanisms that produce those gravitational interactions, and this seems to be what Hume has in mind here. The idea is clearly related to Hume's position that we have no epistemic access to the springs and principles of nature, the ultimate causes behind the regularities of experience. See also THN 1.2.5.26 and its note 12.2: "Nothing is more suitable to [the Newtonian philosophy], than a modest scepticism to a certain degree, and a fair confession of ignorance in subjects, that exceed all human capacity."

Let me put the main point here another way. We may ask a general methodological question: What kind of evidence would be required in order to justify the rejection of a well-established law of nature? Both Newton and Hume set the epistemic bar high. They think we should almost never overturn established laws (because the epistemic burden that is met in order to establish the law in the first place is extremely high), but that we *must* reject a previously well-established law when the appropriate evidence becomes available (in Newtonian terms, when the law becomes known to be "liable to exceptions"). Both Newton and Hume think it will be extremely rare to encounter such situations, but both admit that it will be possible. Newton's account is naturally much more detailed than Hume's, which is to be expected given their different interests.

In laying down a reasonable standard for how new laws are to be established and old laws overturned, Hume is different than, say, Cardinal Bellarmine whose smug contention against Galileo was that plain scientific truths have been established for centuries and we therefore do not need to take new scientific theories like heliocentrism seriously. (Thanks to James Franklin for suggesting this example in personal correspondence.) What Bellarmine did not take sufficient notice of was the extensive evidence Galileo provided to make probable the heliocentric view and to definitively disprove the Aristotelian geocentrism to which Bellarmine dogmatically clung. (We must admit, of course, that heliocentrism was not fully proven, even in a Humean sense, until Newton's Theory of Universal Gravitation definitively settled the case.)

De Pierris (2015) draws this connection between Hume and Newton, too, though she draws the connection more tightly than I would, saying that Hume's account of induction just is or is at least closely modeled upon Newton's. I think I would prefer to say that Hume's account of probable reasoning about matters of fact "bears some remote analogy" to the way Newton uses the method of reasoning from phenomena to establish Universal Gravitation. Multiple, agreeing, independent, precise, and

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

robust measurements of theoretical parameters such as the power law of gravity (the ratio between force of attraction and distance) play the crucial role in Newton's argument, and it is the strength of the conclusions from that sort of incredibly strong evidence that backs up Newton's claim that his account of Universal Gravitation cannot be overturned by mere hypotheses (see Harper 2002 and Smith 2002). Hume's "perfect regularities of experience" are not obviously of the same ilk as Newtonian laws inferred from phenomena via a consilience of precise parameter measurements. Where Hume and Newton agree is that when we have achieved the highest level of empirical proof possible, we should treat propositions so proved as true or very nearly true, regardless of claims to the contrary, until there is evidence of equal or greater kind and strength to overturn them or cause us to revise those laws.

CONCLUSION OF CHAPTER 2

This chapter has examined Hume's general view of testimonial evidence and described the conditions under which Hume thinks it would be possible to accept that a law of nature has been violated. These factors undergird Hume's claim in "Of Miracles" that no testimony is sufficient to establish a miracle. After showing the similarities and differences between Locke's account of testimony and Hume's, and that in Arnauld and Nicole's *Logic or the Art of Thinking*, this chapter explained Hume's account of the psychological mechanism behind the human tendency to give too much credence to the testimony of others, and it argued that the unavoidable use of the Principle of the Uniformity of Nature does not introduce any circularity into Hume's argument against miracles. Hume allows that there could be sufficient evidence for us to revise or overturn an established law of nature, but like Newton he sets the bar for this at an appropriately high level. Hume's position is that the weighing of the evidence on both sides will inevitably lead to the recognition that the evidence for the laws of nature will always be greater than the evidence that a law violation, a miracle, has occurred. The next chapter examines Hume's conception of laws of nature in more detail.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

THREE

Laws of Nature and Reports of Miracles

VIOLATIONS OF REGULARITIES OF EXPERIENCE

An important question to answer in order to understand Hume on miracles is this: To what extent is it possible for there to be violations of—or exceptions to—regularities of experience, or what we might call laws of nature? Hume points out that we sometimes make mistakes about empirical reasoning, especially with regard to empirical generalities and most especially when the regularity that is the jumping-off point for the generalization is not perfect. He writes, "Though experience be our only guide concerning matters of fact; it must be acknowledged, that this guide is not altogether infallible, but in some cases is apt to lead us into errors. . . . [However, experience itself] commonly informs us before-hand of the uncertainty, by that contrariety of events, which we may learn from a diligent observation" (EHU 10.3).

For Hume, since different effects follow from their supposed causes with different degrees of certainty (that is, with different frequencies), in reasoning about matters of fact, experience provides us with all possible degrees of certainty. "A wise man, therefore, proportions his belief to the evidence" (EHU 10.4). That is, one should pay attention to the evidence, and have neither more nor less confidence in the occurrence of some event than is warranted by that evidence. The degree to which one assents to a proposition concerning a matter of fact should be consonant with the balance of evidence in its favor. This matters in the case of miracles because people go on to make inferences *from* claims about miracles—inferences, for example, that some religious doctrine is true, or more likely to be true, because of the miracle. For Hume's purposes it is enough that the occurrence of miracles can only be established so weakly

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

that inferences from them give no reasonable grounds to religious hypotheses, but he thinks an even stronger conclusion against miracles is, in fact, justified.

A report of a miracle is a statement of the occurrence of a matter of fact of a special kind: The event, the miracle, is one that does not occur in the ordinary course of nature nor (*ipso facto*) by natural causes. Hume's basic question is this: *Can there ever be sufficient warrant for believing that a miracle in this sense has actually occurred?* His answer: *Because of the nature of the case, there cannot ever be such warrant*. This is to say that whenever a "wise person" weighs the positive and negative evidence regarding any report of a miracle, she will judge the weight of evidence against the miracle to be (much) greater than the evidence for it.

Hume's argument here is relatively straightforward. A miracle is a violation of the laws of nature. We construct laws of nature on the dual basis of "a firm and unalterable experience"—that is, from an observed constant conjunction of event types, an exceptionless regularity—plus an expectation of the mind that the future will resemble the past. The depth and breadth of the exceptionless regularity of past experience gives the strongest kind of warrant possible to the belief that the law will continue to hold in the same way in the future. It is not that the evidence demonstrates with certainty that the law is true, it is just that no empirical claim can possibly have stronger evidence than what we have with regard to those things we call laws of nature. Testimony, the evidence offered in opposition to the exceptionless regularity, is known to be fallible and is especially suspect in cases of reports of miracles because of the likelihood of deception or misperception. Thus, the weight of evidence derived from testimony about a purported exception to a law of nature in fact will never come close to the weight of evidence from experience that the law will be regular in all cases.

While such a claim can seem a priori—it is hard to read "never" as not having logical force—it is actually analogous to saying, "A human will never bench press 1,500 pounds." That's an empirical claim about a matter of fact, not an a priori claim about a relation of ideas. Given what we know about current records (Kirill Sarychev lifted 735.5 pounds in 2015 to set the "raw" [unequipped] bench press world record), progress over time with improved training (the record was 500 pounds in 1953 and 675 pounds in 1972, for example), human physiology, and the laws of physics (breaking strength of bones, etc.), it is utterly unbelievable that a human (as we currently understand the reference class) could complete a 1,500-pound raw bench press. It isn't logically impossible, just impossible-given-what-we-know. There is a sense in which it is possible that this claim is wrong, but you still should not believe a report that someone has raw-benched 1,500 pounds if you hear one. Notice, by the way, how much *more* unlikely it is that someone walked on water or rose from the dead than it is that someone bench pressed 1,500 pounds. If you would

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

not believe the bench press claim, then on pain of inconsistency you should not believe the miracle claim, either. This way of understanding what Hume means undercuts Larmer's (2009) attempt to support interpretations of Hume which see conclusions such as "testimony will never provide convincing evidence that a violation of a law of nature has occurred" as a priori.

It is worth pausing to note here that there is a danger of importing our own contemporary conceptions of laws of nature into the interpretation of Hume. We should not, for example, imagine that Hume would allow us to consider laws that carry logical, physical, or metaphysical necessity—for Hume, the "necessity" we feel in the laws of nature derives from *our own* contribution to the concept of the law, namely the strength of the *expectation* that our habits of mind attach to an inductive projection based on the degree of regularity in past experience. Likewise, it would be wrong to import our current philosophical ways of speaking, as George (2016) and Earman (2000) do when they speak of "lawlike regularities" instead of laws. This is our contemporary way of saying that laws of nature are something more than mere regularities. The word "lawlike" draws a distinction between a contingent *regularity of experience so far* and some other more robust conception of laws. If the contrast is supposed to be with a "real" regularity (one which will never in fact have exceptions or turn out to fail), then Hume says we can never have knowledge that would ground the distinction. Since the distinction is then empirically empty, we should drop that distinction from our usage when discussing Hume. If the contrast is supposed to be a "real" law in the sense of a causal law with metaphysical necessity, Hume of course rejects the very concept as impossible for us to form. He argues that the "necessity" in a law of nature actually comes from us; it is the psychological *expectation* that future cases will resemble past cases, an expectation that arises unavoidably for creatures like us from regularities of experience. So, we should talk about *laws* not *lawlike regularities* when discussing Hume on empirical generalizations.

VIOLATIONS OF LAWS THAT LACK PERFECT UNIFORMITY

This tells us how Hume deals with laws that involve causal uniformity. But what of causes that are not, or appear to us not to be, perfectly regular? Hume writes, "Tho' there be no such thing as *chance* in the world, our ignorance of the real cause of any event has the same influence on the understanding, and begets a like species of belief or opinion" (EHU 6.1). This is to say that from our epistemic point of view the world can *seem* "chancy" even if it *is* not. In chancy-seeming situations, Hume says that the superiority of chances on one side rather than on another leads to a proportionate degree of belief. Note that Hume is not here

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

assimilating the probability of chances to the probability of causes. Our ignorance of real causes begets "a *like* species of belief," not the *same* species of belief. The two species of belief are analogous in that they are less than perfectly certain, come by degrees, and are subject to non-demonstrative rules of comparison.

It seems that Hume believes that the apparently non-deterministic/probabilistic character of some causes is an epistemological artifact rather than an ontological fact: Hume thinks that causes which appear to act irregularly probably do so because interfering causal factors—perfectly regular ones—exist but are not known to us. Hume writes,

> The vulgar, who take things according to their first appearance, attribute the uncertainty of events to such an uncertainty in the causes as makes the latter often fail in their usual influence; though they meet with no impediment in their operation. But philosophers, observing, that, almost in every part of nature, there is contained a vast variety of springs and principles, which are hid, by reason of their minuteness or remoteness, find, that it is at least possible the contrariety of events may not proceed from any contingency in the cause, but from the secret operation of contrary causes. (EHU 8.13; see also EHU 6.1–4 and THN 1.3.12.5)

The idea that apparent irregularities are to be explained as resulting from unknown but actually regular laws is a methodological rule that is in principle defeasible as inquiry progresses. As De Pierris (2015) puts it, "The guiding idea is that when experience is not completely uniform, we should assume that there are other causes at work which we have not yet discovered. Hume's desideratum, in such a case, is then always to seek the complete causes of the phenomenon so as to come closer and closer to perfect uniformity" (195).

The discovery of truly irregular laws of nature would, however, require the apparent irregularity to be repeated. Singular events such as miracles of the sort with which Hume is concerned could never supply the evidence needed to put aside the regularity. Modern science clearly requires repeatability in order for its observational results to be accepted. This is something Hume's account seems to get right, but it is an aspect of Hume's account that Earman (2000), for example, ignores.

Note that it is very much more difficult to establish that a violation of a probabilistic law, as opposed to a violation of a uniform universal generalization, has occurred. Many significant purported miracles are medical, and medical laws are probabilistic. (Only a certain percentage of people die from a given infection, only some patients recover because of a given treatment, etc.) It would be all the more difficult by Hume's lights to establish that a medical miracle has occurred than that, say, someone has walked on water or turned water into wine. For an event type whose

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

causes appear to us to be irregular, it is nearly impossible to say that the cause of a particular event of that type is outside the order of nature.

The miracle of turning water into wine is partly a matter of *how* the transition is brought about. Give me a vineyard and a year, and even I could turn water into wine. The wedding miracle in the *New Testament* is a miracle precisely because this familiar transition does not take place via the familiar causal pathways. The usual chain of causes (the usual constant conjunction) is claimed not to link the initial and final states. Moreover, the usual effect of waving your hand over a jug of water and wishing—namely that, all of a sudden, *nothing happens*—purportedly does not occur in this case, in violation of our otherwise exceptionless experience. "There are some causes, which are entirely uniform and constant in producing some effect; and no instance has ever yet been found of any failure or irregularity in their operation. Fire has always burned, and water suffocated every human creature: The production of motion by impulse and gravity is an universal law, which has hitherto admitted no exception" (EHU 6.4).

Although some causes may appear to us to be less than regular, it is only the *perfectly* regular ones that really need concern us in the discussion of miracles. In the case of an irregular conjunction of experience (what we might call a statistical law), it becomes nearly impossible to determine if a non-standard outcome is truly a *violation* of such a law. Note, then, that a key feature of a law of nature in this strong sense is that there has never been an experience of the failure of its underlying regularity, not in the history of a given individual epistemic agent and not in the whole history of human experience. Otherwise, it simply is not a law of nature of the kind with which Hume is concerned.

There is no circularity problem here. Hume is not ruling miracles out of court a priori by defining laws so strictly, as some commentators have charged. Miracles defined in terms of some weaker account of laws, where a "miracle" is not necessarily a violation of a perfect regularity of experience, will be susceptible to Hume's critique of design arguments. In that case, the event in question will happen conformably to the order of nature. But in that case miracles defined in this weaker sense would not provide an *additional and distinctive* kind of evidence for religious hypotheses and so they would not need to be treated separately from other design arguments. Hume convincingly argues in the *Dialogues concerning Natural Religion* that design arguments are not sufficient to establish any but the most innocuous, almost contentless, religious hypotheses (as discussed above). The upshot here is that, whether one adopts a weaker or a stronger definition of "miracle," purported miracles cannot provide adequate warrant for religious hypotheses.

Fosl (1999, 181-82) argues that Hume's "Of Miracles" is less convincing than it could be because Hume claims that humankind's experience of the laws of nature is "firm, unalterable and uniform." To some com-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

mentators, this seems to beg the question against experiences that observers take to involve exceptions to laws of nature. Fosl argues that "in fact [Hume's] argument works just as well with weaker, more guarded claims. Indeed, an argument which relies only upon weaker (that is, more limited) premises is a stronger argument" (Fosl 1999, 181). All Hume really needs to claim, according to Fosl, is that the evidence for the laws of nature is the strongest, *most* uniform evidence available, and that it establishes "paradigmatically firm" regularities of nature—our experience, he says, does not actually need to be exceptionless. Since the evidence for laws of nature is the paradigmatically best evidence humans have, the evidence for the occurrence of a miracle can at most equal it, in which case the competing evidences for and against the uniformity of the law in question will balance off, which "must properly only lead us to the *suspension* of judgment on the issue" (Fosl 1999, 182). The conclusion here is consistent in style and substance with Hume's own conclusion, and I am sympathetic to this as an account of how we might in the present day want to argue against believing in miracles, but of course Hume does not himself actually make use of this weaker account of the regularity of experience.

In defense of Hume's stronger claim, I would point out that, even if we allow supposed miracle observations to count as exceptions to the regularity of the particular laws involved, for the vast majority of laws of nature we do have exceptionless experience. No miraculous violations of the law of refraction or of the law of the solubility of sugar in water have ever been reported, for example. Fire burns; bread nourishes. Also, as in the case of using past experience of other inductive regularities to make new inductive projections from singular events (see THN 1.3.8.14), we could perhaps use the exceptionless character of *most* laws as grounds for thinking that *this* law (the one under challenge from a miracle report) is really exceptionless. That is not an argument Hume actually makes, but it strikes me as consistent with his principles and general rhetorical strategies, especially his remarks on how to understand single-case inductions (briefly discussed below).

SINGULAR EVENTS AND SINGLE-CASE INDUCTIONS

For Hume, then, a purported miracle is a purported exception to a hitherto exceptionless regularity. Hume's key question about miracles, again, is this: *Can there ever be sufficient grounds for believing that an exception to a hitherto exceptionless regularity has occurred?* Hume's negative answer follows directly even from just the small part of his empiricist epistemology that has been discussed here.

The very exceptionlessness of regularities of past experience such as are embodied in the law of gravity is what gives us overwhelmingly

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

strong grounds for believing in the future applicability of the law. A subsequent *singular* occurrence, whether positive or negative, affects our overall confidence in the law very little. Hume writes, "The first instance which we saw of motion, communicated by the shock of two billiard balls . . . is exactly similar to any instance that may, at present, occur to us; except only that we could not, at first, *infer* one event from the other, which we are able to do at present, after so long a course of uniform experience" (EHU 7.30). So, for Hume, the warrant of the causal inference is founded on the long course of uniform experience. This suggests that on the first occurrence of an event of *any* event-type (even something that later turns out to be an instance of a perfect regularity) we are *not warranted* in inferring the future occurrence of a similar consequence from a similar antecedent. This in turn suggests that no (other) matter of fact can properly be inferred from a truly singular event. By itself this provides grounds for doubts about miracles.

But is it, by Hume's lights, that one can *never* have a rational belief in a truly singular event? This would be a strong claim. However, this claim is not a consequence, nor is it an assumption, of Hume's position. If the singular event in question happens conformably to the order of nature—that is, so long as it does not appear to contradict any of the laws of nature of which we are aware—then we could potentially have a rational belief that it occurred.

As a quick aside, a related issue bears mentioning here. Hume allows that there are occasions on which we can and do infer a causal law (a university regularity) not from extensive past experience of a constant conjunction of events of the relevant types, but instead from a single occurrence of one thing following another:

> 'Tis certain, that not only in philosophy, but even in common life, we may attain the knowledge of a particular cause merely by one experiment, provided it be made with judgment, and after a careful removal of all foreign and superfluous circumstances. Now, as after one experiment of this kind, the mind, upon the appearance either of the cause or the effect, can draw an inference concerning the existence of its correlative; and as a habit can never be acquir'd by merely one instance; it may be thought, that belief cannot in this case be esteem'd the effect of custom. (THN 1.3.8.14)

Hume quickly points out, however, that this is not really a single-case induction. For, we do not really have *only* the single experience to guide our inference. Rather,

> we have many millions [of experiences] to convince us of this principle; *that like objects, plac'd in like circumstances, will always produce like effects*; and as this principle has establish'd itself by sufficient custom, it bestows an evidence and firmness on any opinion, to which it can be apply'd. The connexion of the ideas is not habitual after one experi-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> ment; but this connexion is comprehended under another principle, that is habitual. (THN 1.3.8.14)

To return now to the main argument: Whether or not we do have a rational belief in the occurrence of a singular event, and just how strong the resulting degree of belief is, will depend for a "wise" person on the kind and strength of the evidence about the event in question. As discussed above, laws of nature are for Hume epistemological rather than ontological categories. This means that some events that are contrary to past experience will seem to be violations of laws, though on further investigation new laws may be discovered with which the event (and past experience) conforms. Once these new laws become known, the event that was originally singular becomes one it is rational to believe occurred. This is as it should be, since for Hume the issue in "Of Miracles" is not what sorts of events do or can occur, but rather what events it is rational, given the available evidence, to believe to have occurred. If the conditions under which the event took place are reproduced and a similar event comes about, that is the beginning of evidence for a new law of nature or revision of an old one. As the evidence of this kind builds up to a high enough level (in which case it becomes increasingly unlikely that there was and continues to be misperception, mistransmission, or deception involved in the testimony and increasingly likely that the event type really occurs), there are then grounds for a rational belief that the original event (although it was singular when it first occurred) really did occur. Degree of belief is for Hume a function of evidence: It will change as the evidence changes.

PURPORTED SINGULAR EXCEPTIONS TO LAWS

So, for Hume, it will be possible in some circumstances to retrospectively acquire grounds for a rational belief in the occurrence of a singular event that at first appears to (but does not really) contradict the laws of nature. Depending on the evidence, it is also possible to have a rational belief in events that are singular but conformable to the laws of nature. One might wonder, however: *Is it possible ever to have evidence sufficient for rational belief in the occurrence of a singular event that is really contrary to the laws of nature?* Hume does not claim that this is impossible. For Hume as for many others in the early modern period, whatever is conceivable without contradiction is possible (see THN 1.2.2.8), and it is conceivable that evidence sufficient for warranted belief in a truly singular event could be discovered. After all, laws of nature are at most probable, never certain, so a contrary instance is not a contradiction and, hence, a contrary instance is conceivable. By the conceivability criterion, then, that means a violation of a law of nature is possible.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Hume does, however, think it extremely unlikely in fact that any such case could be found (that is, acquiring such evidence is not at all probable). Hume's imagined example of the eight days of continual darkness to which there is agreeing testimony from learned observers and careful historians the world over explicitly illustrates that Hume does not rule out the possibility of rational belief in the occurrence of singular events that appear to be contrary to the order of nature:

> For I own . . . there may possibly be miracles, or violations of the usual course of nature, of such a kind as to admit of proof from human testimony; though, perhaps, it will be impossible to find any such in all the records of history. Thus, suppose, all authors, in all languages, agree, that, from the first of January 1600, there was a total darkness over the whole earth for eight days: Suppose that the tradition of this extraordinary event is still strong and lively among the people: That all travellers, who return from foreign countries, bring us accounts of the same tradition, without the least variation or contradiction: It is evident, that our present philosophers, instead of doubting the fact, ought to receive it as certain, and ought to search for the causes whence it might be derived. (EHU 10.36)

Even there, though, Hume thinks that the correct approach will be to look for previously unknown laws or ordinary but unknown conditions that produced the event without there having been a violation of the laws of nature (a volcanic dust cloud, a rogue planet that passes through our solar system and temporarily blocks the sun's light, etc.). Hume is quite sure, moreover, that even if we had sufficient evidence for rational belief in the occurrence of a singular event that resisted analysis in terms of laws, we would nevertheless never (in fact) have sufficient evidence for its supernatural origin. That is to say, practically speaking, that no evidence will ever be sufficient grounds for rational belief in the religious implications of a supposed singular event. This, too, seems to be the correct epistemic attitude.

Earman's example of the simultaneous cloud formations that spell out, "Believe in Emuh and you will have everlasting life," over every nation of the Earth in the language of that nation (Earman 2000, 11), is supposed to be an example of an extraordinary event that would (Earman thinks) give grounds for rational belief in the religious hypothesis written in the clouds. Of course, this need not be a miracle. It might happen that the deity has arranged the laws and initial conditions of wind and weather in such a way that on a certain day the cloud formations in question come about naturally. In that case, there might be grounds for a design argument. Hume does not think much of design arguments in general (see chapter 1), and grants merely that it can be inferred from the natural order that the cause of the natural order (whatever that cause might be) bears some remote analogy to human intelligence. Even so, Hume still might think that Earman's case of the cloud

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

formations would yield a more detailed and more strongly warranted design inference. Whether it is *adequately* warranted will depend on the strength of the evidence that the occurrence was not simply a freak of nature (that it was after all not a product of design but an accident), or that it was not produced as an elaborate hoax (perpetrated, perhaps, by frat boys from outer space).

If the cloud formations do occur in violation of the laws of nature, then this is an instance of a real miracle. One way or the other, the same sorts of evidential considerations must be taken into account before an inference from the event to the religious hypothesis can be made. Hume argues that in all past cases of purported miracles, and in all likelihood in all future cases as well, the balance of evidence has led and will lead wise people to conclude that rather than being a violation of the laws of nature the case in question is one of deception, misperception, or mistransmission.

Hume further concludes that given what we know, including probabilistic facts concerning the reliability of human perception and testimony, we actually do not need to perform this analysis on a case-by-case basis. Hume is not thereby proposing the sort of a priori demarcation criterion Earman (2000, 3–4, *et passim*) sees in "Of Miracles," and which Earman says is an example of the sort of over-reaching that gives philosophy a bad name. Hume's position on this is, rather, a general a posteriori judgment about the facts of experience that are relevant to specific judgments about purported singular occurrences. The core of this is the general claim that a rational person will always in fact (though not in virtue of an a priori commitment) judge that the supposed event did not occur as perceived or reported, or that if more were known it would be clear that the event was not really a violation of any law. That is, we need not perform a detailed analysis in every case and can rely instead on a general (though still a posteriori) position against the credibility of miracles. This is nicely illustrated by the following example (which Ted Morris used in his commentary on an early conference presentation of this work [Morris 2002]):

> If I'm told that a load of tile fell off a semi on the Pennsylvania Turnpike and spilled onto the road in a configuration that replicated Leonardo's *Last Supper*, except that the figure of Jesus looked astonishingly like Elvis, I don't need to investigate further, even though the alleged event violates no natural laws. That is Hume's reaction as well. As he asked Hugh Blair, "Does a man of sense run after every silly tale of witches or hobgoblins or fairies, and canvass particularly the evidence?" (Grieg II, 350) The fact that the truth is out there doesn't mean that *we* have to be out there investigating everything we're told. Part of being wise is knowing when not to waste your time.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

One final aside: Brown (1987) cites al-Ghazali, an eleventh-century Persian philosopher, as someone who argues that proof of empirical propositions such as "cotton burns on contact with fire" or "eating satiates hunger" can only come from experience. Such knowledge is probable, not demonstrative, because God can do anything that is not logically impossible, including making cause and effect relations fail. "[I]t is in God's power to create satiety without eating . . . and so on" (Brown 1987, 662, quoting Averroes 1954, 316). "It is because of this common medieval belief in the possibility of divine intervention in the natural world that such thinkers as Scotus held that generalisations based on experience of particular cases cannot be known as certain" (Brown 1987, 662). It is interesting to note, then, that the "conceivability criterion" invoked by Hume to show that empirical propositions are probable because their contraries are possible has among its roots a medieval view that God has the power to violate the laws of nature at any time.

HUME AND THE REGULARITY THEORY OF CAUSATION

Hume understands laws of nature as general matters of fact, and he analyzes our knowledge of matters of fact in a way that emphasizes that such knowledge depends entirely on the relation of cause and effect. And, of course, he points out that our idea of causation is nothing but an observed regularity plus an expectation of the mind (a custom or habit of thought) according to which we expect future cases to resemble past cases.

Hume's writings on causation, necessary connection, and induction have inspired some interpreters to see Hume as a mere regularity theorist, that is, as holding the view that there simply is nothing to causation except past regularity plus our own expectation. I think this is not quite the correct understanding of Hume's view of causation, and this section is devoted to outlining what I think the correct understanding is.

Strawson (2014, 201) summarizes Hume's position on causation by pointing out that "Hume is certainly making the negative sceptical epistemological claim that (1) we cannot know anything about the nature of Causation in the objects" and Hume is "certainly not making the non-sceptical, dogmatic, ontological-metaphysical claim that (2) there is no such thing as Causation in the objects, and that Causation does not exist." This is meant to be an antidote to the too-common view that says Hume is purely a regularity theorist about causation; that is, the view that says Hume denies the real existence of causation and replaces the typical theory about causal necessity with only with constant conjunction or regularity of experience and an expectation of the mind. I agree with Strawson that Hume himself does not assert that there is no such thing as causation in the world. Hume's point is just that we have no access what-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

soever to whatever causation might be in itself (or "in the objects" as Strawson puts it). So, while Hume is "a strictly non-committal sceptic with respect to knowledge claims about the ultimate nature of reality, he firmly *believes* that there is an external reality . . . and he takes it for granted that (3) Causation does exist in reality, although we are entirely ignorant of its ultimate nature" (Strawson 2014, 201).

Strawson's account here strikes me as exactly correct. The epistemic/ontic distinction is clear in Hume and he is very careful never to confuse the two kinds of claims.

It may be worth mentioning that the Strawsonian way of understanding Hume's position on causation leads rather directly to some things Hans Reichenbach said about inductive reasoning. Reichenbach (1951) pointed out that someone like an inductive skeptic or pure regularity theorist could agree with Hume that there is no rational validation of induction, and yet still find a kind of vindication of induction in our practices and a quasi-evolutionary explanation of how our inductive practices arose. As Reichenbach puts it, either the world is regular, or it is not. If it is regular, then inductive generalization and inductive projection will succeed at formulating laws and predicting future instances. If the world is not regular, no method will find the laws of nature (because, *ex hypothesis*, there are none) and no method of predicting future cases will succeed (because the world is fundamentally random on this hypothesis). In more Humean terms, if the world is in fact regular, then the expectation that the Principle of the Uniformity of Nature holds, will work; if the world is irregular, then nothing we do will allow us to have even probable knowledge of future experiences.

Plus, as Hume himself points out, in life we simply cannot get by without the expectation that the future will resemble the past. Hume even offers a proto-Darwinian account (EHU 5.21-22) of how we (and any species of animal) *should* turn out to expect the future to resemble the past despite having no epistemic access to the metaphysical "powers and forces" of nature:

> [A]s the operation of the mind, by which we infer effects from like causes, and *vice versa*, is so essential to the subsistence of all human creatures, it is not probable, that it could be trusted to the fallacious deductions of our reason. . . . It is more conformable to the ordinary wisdom of nature to secure so necessary an act of the mind, by some instinct or mechanical tendency, which may be infallible in its operations, may discover itself at the first appearance of life and thought, and may be independent of all the laboured deductions of the understanding. As nature has taught us the use of our limbs, without giving us the knowledge of the muscles and nerves, by which they are actuated; so she has implanted in us an instinct, which carries forward the thought in a correspondent course to that which she has established among external objects; though we are ignorant of those powers and

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> forces, on which this regular course and succession of objects totally depends. (EHU 5.22)

I would summarize Hume's position on induction and causation in the following way: Metaphysically, we have no access to any information regarding the reason for the observed regularity in the world. Epistemically, we have no adequate reason to support the expectation that the Principle of the Uniformity of Nature will hold in the future—no reason to support its truth or even its probability. Psychologically, we *just do* expect the future to be like the past. And practically, we cannot avoid assuming the Principle of the Uniformity of Nature is true and we would not be able to get by in the world without that assumption.

The ontic/epistemic distinction so carefully respected by Hume also corresponds to his claims about what it is possible for us to reason and believe in philosophical contexts versus what it is possible for us to reason and believe in ordinary contexts. In philosophical moods, we are perfectly able to be skeptics about induction and causation. In ordinary life, however, it is impossible for us to be skeptics about induction and causation.

So, with Strawson, we may conclude that "Given his strictly non-committal scepticism, Hume is committed to accepting that (5) there may be something like Causation in reality . . . . [Hume] puts forward (3) as something he firmly believes, just as he firmly believes that there is an external . . . world, completely compatibly with his non-committal scepticism with regard to claims about what we can know" (Strawson 2014, 203). Hume believes this, of course, at the level of ordinary life. Hume's skepticism is really more like an antidote to confidence in our rational abilities, rather than a complete undermining of them. Hume's account shows us our limitations in a way that should inspire humility in the claims we make about the world. That is not to say that we should deny natural beliefs like the belief in the existence of the external world or the belief in the resemblance between the future and the past; rather, it is to say that we should proceed in our judgments about better and worse ways of empirical reasoning in the recognition that our cognitive powers and epistemic access to the world do not enable us to come to fundamental and definitive judgments that our beliefs on such matters are absolutely correct.

Strawson later takes this a little farther than I am comfortable taking it, when he writes that what we project (inductively, or in terms of our expectations about real regularity in the world) "is really out there in reality, and its presence out there in reality is in fact the true explanation of why we are so inflexibly disposed to project in the way we do. If we project a concept of Causation onto reality, it's because the presence of Causation in reality has caused us, over millions of years, to become such as to project this way, i.e. to get things right in this way" (Strawson 2014,

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

232). As I say, I cannot go quite this far. Where Strawson writes "Causation" in the passage just quoted, I think Hume would have said "regularity of experience." After all, regularity of experience is all that we have access to, and this regularity of experience is precisely what caused our evolutionary ancestors to develop as the inductive projectors that they became, and we now are. If we have no access to the ultimate springs and principles of nature now, we (or our evolutionary ancestors) certainly did not have such access when perceptual and rational apparatuses would have been *less* sophisticated. Even amoeba inductively project, after all. The regularity of nature is all that is required for creatures in nature to develop a tendency to *act as if* (in our case, *believe as if*) the future will resemble the past—success at expecting how the future will turn out is precisely what promoted evolutionary selection. Without regularity in nature (however that regularity might have come about), there would not have been materials enough for animals and plants to have developed as stimulus-response machines, and certainly our self-aware expectations of regularity continuing in the future would not have arisen.

Still, Strawson is correct that Hume believes, at the level of ordinary analysis, in external reality and real causes. It is just that he simultaneously believes (at the philosophical level of analysis) that those beliefs (at the ordinary level of analysis) have no rational foundation, do not derive directly from experience, and in fact are simply the result of our mind projecting our own expectations on our experience of the world.

For Hume to believe that there is real causation in the world (that there is a mechanism that produces the observed regularity), even though we have no epistemic access to it, is not a contradiction. His position here, in my view, is rather like his inference to limited theism, where he says the available evidence leads us to conclude that the cause of the universe seems to bear some remote analogy to human intelligence, though we can know nothing else about it. We expect that the world is regular, and we cannot help but believe that there are real causes behind the regularity. But all we can reasonably say about those real causes is that they bear some resemblance to our pre-theoretic notion of necessary connection.

Part of Strawson's point is that Hume is not a metaphysical regularity theorist about causation: Hume believes that there is more "out there" than mere regularity, though he cannot justify that belief or spell out its content. But Hume *is* an epistemic regularity theorist, in the sense that all we can *know* about causation, or any general matter of fact like a law of nature, is regularity, and all we can *know* of the necessity of causation is that our belief about it comes from an expectation of the mind, not from the experience itself. We *believe* (perhaps unavoidably) in the "springs and principles" that bring about the continuing regularity of nature, and in that respect, we *believe* in robust laws of nature in a metaphysical sense. These real springs and principles would vindicate our inductive practices if we had access to them, we think. But in our philosophical

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

moods we realize that these beliefs have no rational justification. *Reason* cannot tell us what is really going on in causation, what is responsible for the observed regularity of nature. And yet, in ordinary life, we cannot help but believe and act as if there is real causation in the world. As Hume himself puts it in the *Abstract* of his *Treatise of Human Nature*, "'Tis not, therefore, reason, which is the guide of life, but custom. That alone determines the mind, in all instances, to suppose the future conformable to the past. However easy this step may seem, reason would never, to all eternity, be able to make it" (Hume 2000, 411).

TESTIMONY ABOUT MIRACLES

In the opening paragraph of his attack on belief in miracles, Hume echoes Locke in asserting that a chain of testimony decreases the epistemic warrant for the claim in question more and more as the chain grows in length. He writes, "Our evidence, then, for the truth of the Christian religion is less than the evidence for the truth of our senses; because even in the first authors of our religion, it was no greater; and it is evident it must diminish in passing from them to their disciples; nor can any one rest such confidence in [the Christian fathers's] testimony, as in the immediate object of his senses. But a weaker evidence can never destroy a stronger" (EHU 10.1).

As Locke points out, although some people take it that tradition *increases* warrant, really the opposite must be true (1690, 4.16.10, 664). The campfire game "Telephone" (in which you whisper a sentence in the ear of your neighbor, who then repeats it to the next person, and so on around the circle until it returns to the origin) illustrates rather strikingly the principle of the diminution of the reliability of testimony with the length of the chain of transmission.

We discover by experience that humans have qualities such that most testimony about most sorts of things is normally more or less reliable, though it is nevertheless always possible for testimony to be false by intention or by mistake (EHU 10.5.). Testimony, like subjective experience, provides a *degree* of warrant for a belief—usually lower than the degree of warrant direct experience would have given, but often, as in legal and scientific contexts, sufficient as a basis for belief and action.

Hume and others have pointed out, however, that when the testimony is about a highly unusual event, the reliability of the testimony must be higher in proportion to how unusual the event is. As scientists put it today, extraordinary claims require extraordinary evidence. As Hume puts it, echoing Locke, the incredibility of a fact can invalidate even a very reliable authority (EHU 10.9).

Hume illustrates this with an example borrowed from Locke (probably through Butler: see Beauchamp's commentary on EHU, 247). The

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Dutch ambassador tells the King of Siam that in northern countries water sometimes turns solid enough that an elephant could walk on it. The King, having lived his whole life in the tropics, replies, "Hitherto I have believed the strange things you have told me, because I look upon you as a sober fair man, but now I am sure you lie" (Locke 1690, 4.15.5.2, 657). At EHU 10.10, Hume condenses this story and modifies the details slightly, talking instead about an "Indian Prince." (Perhaps this modification is itself a good example of how details can get scrambled in a chain of testimony.)

One thing this story points out is the context-relativity of the evidential requirements for credible testimony. One's background knowledge in part determines how likely or unlikely one judges some event to be, and thus what degree of evidence one requires in order to believe claims about that event's occurrence or non-occurrence. The same testimony will thus be received differently by hearers with different background beliefs. Given Hume's psychologistic epistemology, this is exactly what we should expect. The context-relativity of the interpretation of testimony also helps explain why some people receive miracle reports as the Gospel truth, so to speak, whereas others are more skeptical. Recognizing the existence of this phenomenon serves as a caution against dogmatic belief, since it could be that we agree with some statements only because they fit with other things we already believe, which themselves may not be well founded.

HUME'S MAXIM

So, what would be required in order for testimony about a miracle to establish that a miracle has actually occurred? According to Hume, unless the falsity of the testimony is more unlikely than is the occurrence of the event in question given our background knowledge, the testimony should not sway opinion (EHU 10.13). Imagine a case, says Hume (EHU 10.37), in which it is reported that Queen Elizabeth is dead, but then a month after the funeral she is found alive and resumes the throne. It would be *much* more likely that some misperception or deception had occurred than that Elizabeth had risen from the dead, even if the falsehood of the testimony to her death seems unlikely because it would require a huge conspiracy of government agents. (The existence of such a large government conspiracy, and thus the falsity of the testimony to the death, may seem more likely to us today than it would have to Hume.) The whole history of humanity concurs in the observation that there is no cure for death. Moreover, there is good reason to think that it is not only possible but even fairly common for people to be misled or to mislead with regard to claims of this kind. As Hume writes, "the knavery and folly of men are such common phaenomena, that I should rather believe

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

the most extraordinary events to arise from their concurrence, than admit of so signal a violation of the laws of nature" (EHU 10.37). It strikes me that a person's reaction to this one line, whether positive or negative, could be the whole difference between those who deny miracles and those who accept them.

Note, too, that the number of witnesses to a supposed miracle does not necessarily increase the probability of the miracle's actually having occurred, either. A huge number of honest and normally reliable witnesses have seen the magician cut his assistant in half—but the event has never in fact occurred. The agreeing testimony of witnesses in this and similar cases provides no grounds for believing that the unusual event occurred, and again this is because of our prior knowledge about how the relevant laws of nature operate. (Though, as Jim Franklin noted in personal correspondence, no one has actually *seen* the magician cut the assistant in half—that is what the box is for!)

All of this said, Hume would still grant the possibility that in some particular case an ancient witness had accurately reported the actual occurrence of a miracle and that the chain of transmission from them to us happened to be entirely uncorrupted. Hume's argument against miracles can accommodate this possibility because it is really an argument against assenting to or believing in miracles rather than an argument against the possibility of miracles occurring. Hume's argument depends upon the fact that in any given instance it cannot be known in advance that the testimony was actually good (that the event was accurately described, and the transmission of the report uncorrupted). Whether or not the testimony is good is precisely what needs to be judged, and Hume says we judge it on the basis of the available evidence and our background knowledge.

Given the vagaries of human psychology and our common experience with other instances of testimony, Hume thinks that we will always, on reflection, judge it far more likely that someone lied or made a mistake than that a miracle actually occurred. As Hume puts it with characteristic irony, we may lay it down as a "general maxim" that "no testimony is sufficient to establish a miracle, unless the testimony be of such a kind, that its falsehood would be more miraculous, than the fact, which it endeavours to establish" (EHU 10.13). Note that, contrary to what many commentators have asserted, this is an outcome rather than an assumption of Hume's analysis of the balance of probabilities in such cases.

An apparently paradoxical implication of all this is that for Hume there may possibly be true facts which it is rational to believe to be false. What makes this rational, and hence not really paradoxical, is a point about epistemic reliability. Miracles either occur or they do not. If miracles do not occur, testimony about miracles will always be false. If miracles do occur, testimony about the occurrence of a miracle will still (given what we know about humanity) almost always be false (by misperceп-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

tion, deception, or mistransmission) and only very rarely true. Considering the set of all cases of testimony, then, it will universally or almost universally be better to believe (the belief will always or almost always be true) that testimony to violations of laws of nature is false. If we follow Hume's maxim we will be in error only very rarely, if at all. (A religionist's objection here might be that we will be in error about the very most important things.) If we follow any other inference rule with regard to miracles we will much more often be led to false beliefs. Because of the tendency to draw implications for religious doctrine from purported miracles, if we follow any inference rule other than Hume's maxim we open ourselves to not just false but dangerous beliefs as well.

The idea behind Hume's maxim is that a miracle report is credible only when it would be more improbable for the testimony to be false than it would be for the law to be violated. Put in the contrapositive, this is to say that it has to be more probable that the perfect regularity of experience was violated than that the testimony was false. It is important to note that a maxim is neither a necessary principle nor a certain truth but is rather (to import some vocabulary from philosophy of science) a "presumptive rule" or "regulative ideal." That is, it is a methodological guideline to be followed in the absence of an adequate reason to think that the guideline does not apply in a given instance. In the case under discussion, the maxim is a rule to guide our reasoning about mutually incompatible evidence claims. It tells us that, barring especially good reasons to trust some particular testimony to a purported miracle, we should withhold assent from purported miracles. The "good reasons" in this case would be definitive evidence that an event did in fact occur in violation of the laws of nature, and that all possible alternative nonmiraculous explanations have been checked and ruled out. These "good reasons" are precisely what Hume thinks will be missing in all practical cases.

Hume's maxim thus functions in some ways like a demarcation criterion but it is in fact not a demarcation criterion. Maxims are defeasible but normally trustworthy guidelines for thought and action. This maxim has it, then, that a miracle report is credible just in case it would be a greater violation of a law of nature for the testimony to be false (through misperception, mistransmission, or deception) than it would be for an otherwise uniform regularity to suddenly fail in a single instance. Given what experience tells us and the facts of the psychology of inductive belief formation, Hume concludes that we will always in fact come to a judgment against the credibility of any miracle claim.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

HUME'S DIMINUTION PRINCIPLE

Hume's general maxim against the credibility of miracles has a second part: A miracle is credible just in case the falsehood of the testimony is more improbable than the event it describes, "and even in that case, there is a mutual destruction of arguments, and the superior only gives us an assurance suitable to that degree of force, which remains, after deducting the inferior" (EHU 10.13). Hume's point here is that even if one finds oneself able to judge that the falsehood of the testimony is more unlikely than the falsehood of the event to which someone is testifying, one must still compare the probabilities on each side. The extent to which the falsehood of the testimony is probable is to be "deducted" from the probability that there was a violation of a law of nature. The difference remaining is the degree of assent to be attached to the miracle. Since the probability of the non-violation of a law is very high, the probability of the truth of the testimony will have to be very high as well (in fact slightly higher), and the difference between the two, which establishes the degree of probability we should attach to the miracle's occurrence, will thus be very small.

An analogous case is that of a criminal trial in which there is one highly reliable witness against three agreeing but individually less reliable witnesses. The jury must weigh the competing probabilities to come to a decision. The decision's degree of certainty will not be very high in the end, because the competing testimonies partly cancel each other out. Depending on the details of the testimony, the witnesses's reliabilities, and the other evidence in the case, the jury could decide that the first testimony is the one to believe, that the second is, or that they do not know what the correct story is.

Earman calls this second part of the maxim Hume's "diminution principle," and it comes in for serious criticism on Bayesian grounds. In fact, the second part of Hume's maxim regarding miracles has long been a cause of concern, because it seems that Hume is double counting (cf. Earman 2000, 43). The degree to which one should believe that the next roll of a die will come up six is not calculated by subtracting the probability that the event will not occur from the probability that it will occur: 5/6 – 1/6 = 4/6 against, or 2/6 in favor. Clearly the correct degree of belief is 1/6 in favor.

Hájek (2008) discusses criticisms of Hume's diminution principle (Hájek calls it Hume's balancing principle). The famous "lottery" example that originated with Richard Price (in his *Four Dissertations*, first published in 1777) is used by Hambourger (1980) to argue that Hume's diminution principle cannot be upheld:

> He [Hambourger] imagines a lottery in which there are one million entrants, and that the *New York Times*, reports that the winner of the

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> lottery is Smith. He supposes that the *Times* misreports the winner one in ten thousand times. Hambourger says "it is even more unlikely that Smith should win than that the *Times* should make a mistake, and, therefore, on balance, it is more probable that Smith lost the lottery, and the *Times* misreported the winner, than that Smith won the lottery" [Hambourger 1980, 592]. Thus, it is an unwelcome consequence of Hume's balancing principle that the testimony of the *Times* should not engage our belief—or so Hambourger argues. Thus, the principle proves too much and should be rejected, according to Hambourger. (Hájek 2008, 95)

Hájek (2008) goes on to say, "I do not find these criticisms to be damaging to Hume's balancing principle. The 'unwelcome consequences' of the principle, while undoubtedly unwelcome, are in fact not consequences of the principle at all" (95). Hájek argues that although the probability of Smith winning the lottery may have been 1 in 1,000,000 *before* you had the *Times* report, *after* the *Times* report there is just a 1 in 10,000 chance that Smith did not win. That is, 9,999 times out of 10,000 in these sorts of circumstances, you will be correct in believing that Smith won. The lottery example is *not* a "counterexample to Hume's balancing principle concerning the probability you give now to Smith's winning, post report (which is $1 - 10^{-4}$), without the wildly implausible premise that you cannot rationally update your probabilities. That would really prove too much!" (Hájek 2008, 18). All Price-style objections to Hume's diminution principle suffer the same structural flaw by Hájek's lights.

Hume's discussion of the balance of "proof against proof" is clearly incompatible with the "complementational rules of negation, addition and multiplication" (Coleman 2001, 198), including the idea that the sum of the probabilities of all the mutually incompatible contrary possibilities equals 1. As I will show below, this is the source of Bayesian/Pascalian critiques of Hume's Diminution Principle, including Earman's. Instead of thinking of a finite quantity of probability (totaling up to certainty) being "spread over" all the contrary possible outcomes, think of a balance beam: One can keep piling probable evidence on each side, and there will be many different states (with different absolute "weights" of evidence) which yield equilibrium or near equilibrium, that is, doubt and suspension of belief. (Especially since the weight of evidence does not add linearly: the thousandth instance counts for less than the fourth.)

So, Coleman offers a different resolution of the apparent difficulty in Hume's diminution principle, essentially sidestepping the objection. In effect, she argues that the simple mathematical example used above—and we could add, Earman's more complex Bayesian ones—are not appropriately analogous to Hume's reasoning about evidential probability. If this is correct, and I argue that it is correct in later chapters, then any attempt to give a mathematically-based counterexample like Price's will fail to hit home, even if Hájek is wrong about all such examples being

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

flawed in the same way. I say much more below about the principles behind Coleman's defense of Hume's diminution principle.

THE TESTIMONY OF FRIENDS AND PERSONALLY OBSERVED MIRACLES

The story so far, like "Of Miracles" itself, considers only cases involving testimonial reports of miracles that come from tradition, strangers, and written accounts. Would the conclusion be different if we were considering, as Hume does not directly do, a case of testimony to a miracle by a close and trustworthy friend or a case of witnessing a miracle for oneself?

O'Brien (2007) contends that Hume's account of sympathy would lead him to contradict his general account of testimony in instances where friends claim to have witnessed things that would be otherwise too improbable to believe were they reported by strangers. O'Brien argues that Hume's account of sympathy leads him to be partial to the testimony of friends and to believe what they say almost always and especially on certain kinds of matters, despite what the probabilities might tell us from an evidentialist point of view. On this interpretation it is, moreover, a kind of moral failure (a failure of the duties of friendship) to doubt the testimony of friends. So, by O'Brien's lights, it is not just that we have a natural tendency arising from sympathy to believe whatever our friends say, but that we *ought* to believe what they say. The force of the "ought" here is both moral and epistemic.

O'Brien navigates the apparent contradiction between Hume's evidentialist account of testimony and his partialist account of the testimony of friends by pointing to Hume's naturalism. Just as we cannot avoid reasoning in accordance with the Principle of the Uniformity of Nature, we cannot avoid giving credit to what our friends tell us. Since Hume gives normative weight to ways in which we cannot avoid thinking (for example, with regard to induction), this suggests that it is normatively correct to believe friends who testify to otherwise-improbable matters of fact.

I cannot quite accept O'Brien's conclusions. I do not agree that Hume would assent to a friend's testimony to a miracle. Hume would, I think, because of the duties of friendship, try to resist publicly contradicting his friend and would avoid making the friend look bad. He would, too, try to think well of his friend's intentions and abilities, and attempt to come up with a reasonable interpretation of the formation of the friend's belief that he or she witnessed a miracle, an interpretation that does not make the friend look ridiculous. Part of the duty of friendship might also include taking the friend aside and gently but firmly prompting the friend to reexamine what the friend purports to have witnessed.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Just as other passions such as awe and wonder can affect the vivacity of and hence the degree of belief in certain propositions, the passion of sympathy arising from friendship can do the same. The beliefs arising from those passions need to be corrected by reason, and the same is so in the case of the testimony of friends. It is true that we do use the Principle of the Uniformity of Nature, and that we do tend to believe our friends. It is true that we cannot avoid using the Principle of the Uniformity of Nature; but it is *not* true that we cannot avoid believing our friends even when they report improbable things. Proof of this is that we do, in fact, doubt our friends in at least some such cases. (How we *act* toward our friends in cases when we disbelieve them is where the moral dimension enters the picture.)

What would Hume say about cases in which an individual personally witnesses something that seems to be a violation of a law of nature? Dumsday (2008) argues that personal religious experience, even inter-subjective personal religious experience, could be acceptable evidence for belief in the occurrence of a miracle even for Hume. Since there is no testimony in such a case, Hume's maxim is not in play. (Hume's maxim, again, is the principle that a miracle report is believable only when the testimony about it makes the falsehood of the report more unlikely than the falsehood of a previously exceptionless regularity of nature.) Under these conditions, would Hume's conclusions about the irrationality of belief in miracles change in any way? After all, there is a strong tendency to give priority to the "direct testimony" of the senses. When we see it with our own eyes we usually cannot help but believe it; ideas derive directly from impressions in such cases.

Nonetheless, Hume's answer would still be that we should disbelieve miracles even when it seems we have witnessed them for ourselves. Hume is very aware of the fallibility of perception. As he notes, in general we know that we make mistakes when observing, and that we are more likely than usual to misperceive in precisely the kinds of situations in which miracles are claimed to occur. Given the uniformity of past experience, Hume would further suggest that in the case of a purported "personal miracle" where we feel confident in ruling out misperception, it would still be better to think that some previously unknown law or causal factor led to an *apparent* law violation that was not really a law violation. This is what we think when we see a street magician; why should it be different for other kinds of cases where we observe an apparent law violation? In short, there are no personal or personal-friend exceptions to Hume's skeptical conclusion regarding belief in miracles.

Levine takes this to be a fatal flaw in Hume's account. He writes,

> If a resurrection were well enough attested to warrant belief, then that event could still only be assigned status as an extraordinary event with a natural explanation. Hume is thus constrained by his empiricism. He

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> is constrained in such a way that had he been at the shore of the Red Sea with Moses when they were being chased (as in the C. B. De Mille movie version); and had Moses raised his staff and the Red Sea split up the middle (i.e., no low tide but raging waters on both sides); and had the Red Sea crashed to a close the moment the last Israelite was safe—killing those in pursuit; and had Hume himself lacked grounds for assuming he was hallucinating or perceiving events in any way other than as they were actually happening—Hume would still be constrained by his principles to deny that what he was witnessing was a miracle. This example suffices to show the unacceptability of Hume's argument. (Levine 2002, 163-164)

Part of the rhetorical force of Levine's example comes from the familiarity of the story to most Judeo-Christians. That familiarity, it seems to me, makes us less able to realize just how ludicrous the story is. The Red Sea is not narrow. Even the "skinny" northern projections are about twenty miles across. A well-fed, well-supplied, and well-disciplined modern army can march about thirty miles in a day. A ragtag band of hungry, hot, and frightened civilians, including children and the infirm, carrying everything they own, would surely take at least a day if not more to cross twenty miles. Near Suez, where the Red Sea narrows to ten or so miles across, surely it would have been easier to go around rather than through. If the point were merely for God to smite the Egyptians so the Israelites could escape, surely he did not need an ocean to do it; he could have used any other method (simultaneous heart attacks, a vicious and highly localized sand storm, killer bees, etc.). Levine's assumption that Hume as witness to the Red Sea parting could know for sure that he was not hallucinating or misperceiving, is one Hume simply would not accept. Surely heat stroke, a fever brought on by bad food, or a mirage is all much more likely than that a ten- or twenty-mile stretch of water would behave that way. That, or there was some natural-but-unknown process that temporarily lifted the sea floor at a lucky moment. (Not so lucky for the Egyptians.)

To my mind, it is a virtue, not a flaw, in Hume's account of miracles that even in cases where we personally witness an apparent violation of the laws of nature, Hume cautions us to consider alternative explanations, weigh the competing evidence, and skeptically analyze the claims in question. Doing so, we will almost always conclude that we do not have sufficient grounds to believe that a miracle has occurred. This is the conclusion any reasonable person should hope for in such cases.

Hume himself ends "Of Miracles" with considerations not unlike those just mentioned. That religious belief is and must be founded on faith rather than reason, Hume demonstrates by asking us to consider just the miracles in the Pentateuch:

> Here then we are first to consider a book, presented to us by a barbarous and ignorant people, written in an age when they were still more

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> barbarous, and in all probability long after the facts which it relates, corroborated by no concurring testimony, and resembling those fabulous accounts, which every nation gives of its origin. Upon reading this book, we find it full of prodigies and miracles. It gives an account of a state of the world and of human nature entirely different from the present: Of our fall from that state: Of the age of man, extended to near a thousand years: Of the destruction of the world by a deluge: Of the arbitrary choice of one people, as the favorites of heaven; and that people the countrymen of the author: Of their deliverance from bondage by prodigies the most astonishing imaginable: I desire anyone to lay his hand on his heart, and after serious consideration declare, whether he thinks the falsehood of such a book, supported by such a testimony, would be more extraordinary and miraculous than all the miracles it relates; which is, however, necessary to make it be received, according to the measures of probability above established. (EHU 10.40)

CONCLUSION OF CHAPTER 3

This chapter has shown that Hume's account of laws of nature is robust and can do all the work we want it to do. As empirical matters of fact, law statements are never demonstratively certain. They can be overturned or revised in light of sufficient evidence. Hume also has a plausible account of how we can form what look like single-case inductions. His methodological principle that we ought to look first for naturalistic explanations of apparent law violations is consistent with wise sentiment and good scientific practice. His insistence on a high standard of evidence for overturning an otherwise-exceptionless regularity is likewise sensible. Given these and other parts of Hume's account of laws of nature, it also makes sense that we should not bother to pursue in detail even claims about remarkable coincidences that are not law violations, let alone spending much time analyzing claims about purported law violations.

Also, testimony about miracles in particular cannot meet the epistemic standards necessary for overturning well-established laws. Hume's view about testimony to violations of the laws of nature is that it is far more likely to arise from misperception, deception, or mistransmission than it is to be true; the evidence for laws of nature is very strong indeed. Hume cautions us therefore not to accept the occurrence of a miracle based on the evidence of testimony. The first part of Hume's maxim, that we should only believe the occurrence of a miracle when the falsity of the testimony to it would be more unlikely than the violation of the law of nature that testimony reports, is a high but appropriate standard. The second part of Hume's maxim, the Diminution Principle, is clearly incompatible with fundamental principles of mathematical probability, but this is no problem for Hume since (as we will see in later chapters in

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

detail) he adopts a deliberately non-mathematical approach to probability and it is inappropriate to impose that framework on his argument. Hume's maxim does not assume the case against miracles but is rather a defeasible regulative ideal built on Hume's basic empirical epistemology. Hume does not directly address the issues of miracle testimony provided by friends or the evidence from personally witnessing a purported miracle, but I have argued here that in both types of cases Hume would be able to consistently maintain that still we do not have sufficient evidence for warranted belief in the occurrence of a violation of a law of nature.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

FOUR

Hume and the History of Evidential Probability

HUME AND THE HISTORY OF THE NON-MATHEMATICAL APPROACH TO EVIDENTIAL PROBABILITY

In her landmark work, *Classical Probability in the Enlightenment*, Lorraine Datson (1988) has as her main focus chasing down an answer to the question of which concepts of probability became quantified, when, and how, and what happened to them as a result. She does not address the question of which kinds of probabilities *can* and *should* be quantified. Datson sets her project as investigating "the intersection between the qualitative and the quantitative" (6). But if this is a Venn diagram analogy, the non-emptiness of the intersection tells us nothing, by itself, about the other regions of the diagram, for example, about qualitative probability that is not quantifiable, or quantified probability that cannot be properly expressed qualitatively. The intersection between qualitative probability and quantified probability is powerful and has occupied a good deal of rigorous thought for several hundred years, but I suspect it is not the largest region of the diagram. This point aside, Datson's book is a crucial contribution to the history of philosophy. In her introduction, she writes,

> While conceding the skeptical claim that absolute certainty lay beyond human grasp in all but a very few areas, [a loosely related group of seventeenth-century thinkers] asserted that the conduct of daily life furnished sufficient, if imperfect standards for moral certainty. It was rational to believe or act in religious or philosophical matters if comparable evidence would persuade a 'reasonable man' to adopt a course of action in his daily affairs. This pragmatic rationality of partial certainty contrasted sharply with the demonstrative certainty demanded

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> by the Scholastics and later by the Cartesians. Between the poles of absolute certainty and total doubt, the reasonable man interpolated and compared degrees of certainty. These were the probabilities that the mathematicians sought to quantify in their theory. (Datson, xi-xii)

Note that these probabilities were not already mathematical: The mathematicians in question *sought* to quantify these probabilities. Note, too, that the initial point here, that there is evidence sufficient for moral certainty, was not a claim that originated with these thinkers (Mersenne, Gassendi, Locke, et al.) but rather was one that was widespread throughout European thought at the time. (One could add that even Descartes accepted that probable reasoning is all that is humanly possible outside of metaphysics and mathematics—a conclusion that led Descartes to instrumentalism rather than realism in science, as is clear in the final parts of his *Principles of Philosophy* [1985 (1644)].)

Thinkers in the early modern period were very interested in questions about probable evidence and probable reasoning: "Seventh-century texts—literary, religious, philosophical, medical, scientific, legal—abound with references to 'probability' of one sort or another" in "proliferating, mutating uses" (Datson 1988, 4). However, "Recasting ideas in mathematical form is a selective and not always faithful act of translation" (Datson 1988, 4–5). *True dat, son.* As her book demonstrates, "[O]nly some of the ambient seventeenth-century views about what probability meant passed through the filter of the mathematical methods invented [in this period]. Those that did changed their meaning as well as their form. . . . Quantification was not neutral translation" (Datson 1988, 5). This is an excellent point that historians of ideas would do well to keep at the top of their minds when studying probability in the early modern period. Bayesians, too, who attempt to "translate" non-mathematical evidential reasoning into mathematical form should beware that doing so in fact transforms the material they are trying to represent. This is part of my diagnosis (in chapter 5) of why the many Bayesian attempts to capture Hume's argument against miracles are so different from one another (and from Hume).

Datson argues "that seventeenth-century legal practices and theories shaped the first expressions of mathematical probability and stamped the classical theory with two of its most distinctive and enduring features: the 'epistemic' interpretation of probabilities as degrees of certainty; and the primacy of the concept of expectation" (Datson 1988, 6). Datson (1988) claims, "that more than any other single factor, legal doctrines molded the conceptual and practical orientation of the classical theory of probability at the levels of application, specific concepts, and general interpretation" (6). She goes on to list a series of commentators who mention the legal connection in passing but tend to dismiss it. Datson is arguing for the explicit and important connection of legal probabilities to the devel-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

opment of the mathematical concept. I am in complete agreement on this point. Datson does not trace the roots of the legal concept of probability (as Franklin [2001] traces it to Roman law), and her interest is not in the parts of the legal concept that cannot or should not be quantified.

Datson (1988, 10) makes the interesting claim that it was the lack of the development of the concept of true randomness which at first held back the quantification of probability, and that this lack can be traced to a conceptual block: Thinkers in the period could not accept that some things were indeterministic, uncaused. "The random was simply unintelligible" (11). This, Datson points out, did not hinder the development of a *qualitative* account of probability. Moreover, I would add, this "conceptual block" explanation of why legal probabilities were not quickly quantified, would not apply to Hume, who clearly does not make the assumption of necessary causation and determinism in his philosophies of evidence, laws of nature, causation, and so on.

A different approach to understanding the gap between qualitative and quantitative probabilities can be found in Dorothy Coleman (2001). Coleman defends Hume's diminution principle, the idea that even in the case of reliable testimony regarding a miracle, when the testimony is compared to the experience of an otherwise exceptionless law of nature, there is "a mutual destruction of arguments, and the superior only gives us an assurance suitable to that degree of force, which remains, after deducting the inferior" (EHU 10.13). While conceding that the diminution principle is inconsistent with mathematical probability theory (as Earman 2000, 49-53, shows), Coleman effectively argues that Hume can evade a critique like Earman's because Hume's theory of evidential probability has an entirely different structure and basis than that of mathematical probability. Using a distinction developed by Cohen (1980), Coleman categorizes Hume as a "Baconian" rather than a "Pascalian" about evidential probability: "[W]hat all conceptions of probability have in common is that they provide different criteria for grading degrees of *provability*, and that degrees of provability allow for two kinds of scales. Pascalian scales take the lower extreme of probability to be disprovability or logical impossibility; the Baconian scale takes the lower extreme to be only non-provability or lack of proof" (Coleman 2001, 198).

In other words, for a Pascalian the fact that a proposition has "zero probability" means that the proposition is a logical impossibility, whereas for a Baconian the fact that a proposition has "no probability" means that there is no positive evidence in favor of accepting it. When a proposition has maximum probability for a Pascalian, this means it is a logical truth (its falsity is logically impossible); for a non-Pascalian, "full proof" regarding an empirical fact is less than perfect certainty, even though full proof is the highest degree of probability attainable for empirical propositions.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Coleman's understanding of Hume on these points is certainly consistent with my reading of his remarks on evidential probability. Often, Hume is concerned with probability when he is evaluating the bases for various empirical beliefs; in those contexts, a scale running from "not proved" through "proved" is exactly what is needed, especially for a skeptic like Hume. If it is true that Hume is a non-Pascalian about evidential probability, then his diminution principle raises no internal inconsistency.

Today probability is almost always discussed in terms of the mathematical theory of probability; even when numbers are not explicitly invoked, the tacit understanding seems to be that numbers could or should be used, if we were to give a fully rigorous account of the case. Blaise Pascal is credited with having developed the mathematical theory of chances in 1654; Christian Huygens published the first set of axioms of mathematical probability in 1657, and A.N. Kolmogorov gave us the first fully general axioms of mathematical probability in 1933. Pascal's initial work (in a series of letters with Fermat) focused on "the problem of dice," namely the problem of deciding the following question: "If one agrees to throw a certain number in a given number of throws, does one have the advantage?" The mathematical theory of probability was quickly extended to other games of chance, and it has since also been extended to many other kinds of problems as well—for example, deciding whether or not the risk associated with being inoculated against small pox is worth the benefit, how annuities ought to be structured, and even evaluating the probability of scientific hypotheses in light of the available evidence.

In his 1975 book *The Emergence of Probability*, Ian Hacking asserted that there was no conception of evidential probability before the seventeenth century. Scholars have since shown that Hacking was wrong about this (see Garber and Zabell 1979) and about some of his other historical claims about probability (see below), but the entire story had not been fully told until James Franklin's book, *The Science of Conjecture: Evidence and Probability before Pascal*, appeared in 2001. Despite the fact that it is a very good book with important things to say, it has not received the attention it deserves from scholars. (Franklin [2001] was reviewed in a few law journals, for example, Gordley [2002], and statistics journals, but the history of science journal *Isis* did not review it until 2005. The philosopher Ian Hacking wrote that review, and it is positive. I have not seen Franklin's work mentioned by any other philosophers.)

Cohen claims that "We must apparently look to David Hume . . . for the first explicit recognition by one of Bacon's admirers that there is an important kind of probability which does not fit into the framework afforded by the calculus of chance" (Cohen 1980, 225). He argues that there is "a long line of philosophical or methodological reflections about such a probability, stretching at least from the seventeenth into the nineteenth century" (Cohen 1980, 219). This approach is present, for example, in the

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

philosophies of science of J. S. Mill and J. W. Herschel, and in the legal theory of James Glassford, a nineteenth-century Scot.

Cohen's basic claim seems right, but there is something odd about the details. Hume was, after all, largely echoing Locke's view of probability, so Hume is not the first writer after Bacon to hold that theory of probability. And if Hume was the first to *explicitly* distinguish the two conceptions of probability, he did so in a way that has been missed by most of his readers. As I show below, what Cohen and Coleman call the "Baconian" approach to probability in fact predates Bacon by centuries. The long tradition in which Hume was participating may be one reason Hume did not feel the need to make his theory of probability explicit; it was already well understood in his milieu and he only needed to invoke its principles. As it turned out, a historical accident, namely the rise of mathematical probability just at the time Hume was writing, obscured the entire tradition. Later commentators lost sight of that ancient tradition of probable reasoning, and thus often fail to accurately understand Hume's positions on evidential probability. These historical considerations lead me to drop the name "Baconian" and instead refer to the "non-Pascalian" or "non-mathematical" tradition of probability.

Now, there is a sense in which a defense of Hume's diminution principle is unnecessary to his overall conclusion against the credibility of miracle reports. That is because when Hume speaks of the battle of proof against proof, he is speaking counterfactually: He actually thinks that the evidence for a miracle will never reach the level of proof. Wanting to cover his bases, however, he introduces the diminution principle in order to take account of any objections that might begin from the assumption that the evidence for a miracle amounted to a proof. *Even in that case*, Hume says, the degree of belief we should have in the miracle is small because it must be balanced off against the evidence for the uniformity of the laws of nature.

Coleman's defense of Hume's diminution principle is a good one unless there are definitive arguments against the non-Pascalian conception of degrees of proof. This is an issue I cannot conclusively resolve here, but I claim that the non-Pascalian approach is at the very least plausible for the kinds of cases Hume is considering. In what follows I support this claim in two ways. Below, I show that Hume's approach is consistent with a long tradition of thinking about evidential probability, a tradition that is plausible in its own right. In chapter 5, I defend the non-Pascalian approach and argue that it is not ruled out by the "Dutch Book" argument, an argument various authors use to try to show that one *must* reason about probability in Pascalian terms.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

THE HISTORY OF PROBABILITY BEFORE PASCAL

For more than 1,300 years, from the founding of Rome in 753 BCE to the death of Emperor Justinian in 565 CE, Roman law was officially in effect throughout the Empire, and "Roman law is still at the heart of the civil legal tradition of the European continent and some of its former colonies in the Americas, Asia, and Africa, and it was instrumental in the development of international law, the church's canon law, and the common law tradition. Roman lawyers created new legal concepts, ideas, rules, and mechanisms that most Western legal systems still apply" (Domingo 2018, 3). With a 2,800-year reach, Roman law has to be considered one of humanity's greatest intellectual achievements.

From 533 CE, the *Digest* and the *Institutes* (a textbook for beginning law students) formed the legal foundation for the entire empire (Domingo 2018, 81) and they officially replaced all prior ancient legal writings. The *Digest* was rediscovered in the west in the eleventh century and from that point on, "Justinian's Roman law permeated all European legal systems to different degrees and at different times. . . . The fusion of Roman law, canon law, and feudal law produced the *ius commune*, a common legal system in Europe in force until [about 1900]" (Domingo 1988, 88). Though the legal systems of modern countries are no longer explicitly tied to Roman law, it has remained influential as a touchstone of legal interpretation, especially in civil law.

For present purposes, what is relevant about the ancient Roman legal tradition is its treatment of evidence, testimony, and probability. Franklin (2001) discusses evidential probability from its roots in ancient Greek and especially Roman thought, through the medieval period and up to the seventeenth century. Franklin begins with a standard distinction between two categories of probability. The first is "factual," "stochastic," or "aleatory" probability; it has to do with chance setups (such as rolling dice) that produce random sequences. The second is "logical," "epistemic," or "evidential" probability, the theory of the relation of partial support between propositions, also known as non-deductive logic (Franklin 2001, *x*). Franklin shows that the two kinds of probability have been treated separately throughout almost the whole of the history of western thought. Moreover, "while the probability of outcomes with dice throws is essentially numerical, and advances in understanding are measured by the ability to calculate the right answers, it is otherwise with logical probability. Even now, the degree to which evidence supports hypotheses in law or science is not usually quantified, and it is debated whether it is quantifiable even in principle" (Franklin 2001, *xi*).

Bayesianism assumes that both factual and epistemic probabilities are to be treated in the same way: numerically. Bayesian attacks therefore turn, in effect, on accusing Hume of not treating the evidence for miracles in the same terms as dice throws are treated. Given that the debate over

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

the nature of probability is even now not settled (see Gillies 2000, for example), and given that history sides with Hume in treating factual and epistemic probabilities as different from one another, Earman's contention that "Of Miracles" is an abject failure is clearly far too strong.

Ancient and medieval thinking about probability is mostly in the context of the law where, naturally, there is a concern about what kinds of evidence establish what kinds of facts with what degree of assurance. The Talmud and Roman law are similar with respect to rules of proof, especially on the question of witnesses and in the fact that a high standard of proof is required for conviction. This latter point is related to the judgment that it is morally worse to convict the innocent than to acquit the guilty. Because of this, in turn, "The fundamental rule in Roman law, and since, [is] that 'proof is incumbent on the party who affirms a fact, not on him who denies it'" (Franklin 2001, 7, quoting the Roman *Corpus of Civil Law*).

The Gregorian Reform of the twelfth century was led by logic and law (Franklin 2001, 15), and one of the most important texts in this period was the *Digest* of ancient Roman law, re-discovered in Pavia about 1050 C.E. Members of the first generation of medieval commentators on the *Digest* are known as "Glossators"; modern law descends from the Glossators in an unbroken tradition (Franklin 2001, 16). In contrast to Roman law itself, which was focused on particular cases and heavily dependent on the judgment of individual jurists rather than being theoretical, the Glossators and later legal commentators had a taste for generalizations. Many of their general claims have to do with "presumptions," rules for reasoning about things held to be true unless there is proof to the contrary. These include rules for how to treat different kinds of circumstantial evidence and how to balance conflicting evidence. (See Franklin 2001, 17, *et passim*.)

The Glossators initiated the development of thinking in terms of (non-numerical) *grades* of probability when they invented the concept of "half proof." In ancient and medieval law, two witnesses or a notarized document were normally required for "full proof," that is, for proof sufficient for conviction. The principle appears first in Deuteronomy ("A lone witness is not sufficient to establish any wrongdoing or sin against a man, regardless of what offense he may have committed. A matter must be established by the testimony of two or three witnesses" [DT 19: 15]), and the concept is also found in Roman law, summarized by the Latin slogan "*testis unis, testis nullus*" or "one witness is no witness." The Glossators's innovation was to come up with a way to deal with the fact that a single witness, or a private document, is not entirely evidentially worthless even though it is still less than full proof. The Glossators proposed that two half proofs added together would be sufficient for conviction although one alone would not be. Franklin writes,

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> The medieval lawyers were the first to consider explicitly the grading of degrees of proof, with some discussion of how fine it should be; the combination of different pieces of evidence bearing on the same conclusion; and the conflict of different pieces of evidence bearing on the same conclusion. The resulting theory is a coherent one. It is not numerical, and there is no reason to think it would have been improved if it had been numerical. On the contrary, since modern (English) law has a similar theory, and insists on keeping it non-numerical, there is every reason to believe the medievals were correct in avoiding numbers. (Franklin 2001, 12)

As Franklin (2001) notes, there were a few attempts to discuss finer grades of legal proof in a numerical fashion, but they were dead ends. For example, a passage from the *False Decretals*, a hodgepodge of real and forged documents assembled around 850 C.E., asserts that seventy-two witnesses are required to convict a bishop, forty-four to convict a priest, and so on down to the doorkeeper, who needs seven. This "is the world's first quantitative theory of probability. Which shows why being quantitative about probability is not necessarily a good thing" (Franklin 2001, 13–4). For the most part, numerical and even quasi-numerical accounts of evidential probability did not appear until after Pascal.

Among the Roman and medieval principles of evidence relevant to the discussion of Hume are the following: It is better to acquit the guilty than to convict the innocent; a higher standard of evidence should be required for the authentication of relics or for canonization (Franklin 2001, 22); and in general "A stronger proof is required from one who wishes to prove what is not likely" (Franklin 2001, 27, quoting from the *Decretals* of Gregory IX of 1234). Obviously, this way of construing the burden of proof is completely consistent with both Hume's general account of reasoning about evidence and his specific remarks on miracles. The familiarity of these ideas even today shows their long and steady influence on how we think about evidence and warranted belief in the Western tradition. Today, we adapt and adopt these essentially legal concepts in our own thinking about empirical evidence, just as Hume and his contemporaries did, too.

An important thing to notice about the sorts of prescriptions about evidence just mentioned is that rules of prudence, not just schemas of calculation, enter into the evaluation of evidential probability. Rules of prudence depend on *values*—epistemic values but also, and significantly, ethical values.

One relevant example of the influence of rules of prudence in evidential reasoning in the law is the rule that it is better to acquit the guilty than to convict the innocent (an ethical rule of prudence) balanced against the rule that the guilty should be convicted (another ethical rule) while at the same time recognizing that perfect evidence is practically

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

never available (an epistemic fact of life). All together, these ideas yield the doctrine of "proof beyond a reasonable doubt."

The legal doctrine of proof beyond a reasonable doubt codifies a standard of evidence that is lower than perfect certainty but still a very high degree of probability, so high that we are confident in acting in very serious ways on this conviction. In short, we *act as if* we have perfect certainty, though we may mentally reserve the slight but not significant possibility of being mistaken. This is quite different than a radical skepticism, and it is certainly not dogmatism. One might go so far as to assert that being able to reason in this careful, nuanced way is constitutive of being a civilized, reasonable person.

The clear connection between the two meanings of "conviction"—in the sense of *being found guilty* and "conviction" in the sense of *being fully convinced or having full belief*—is present in the Latin roots as well as in the English words. Interestingly, it is the legal sense which seems to have come first, and then later the mental state came to be referred to by the same term.

In science, the analogy to acquitting the guilty and convicting the innocent is confirming false hypotheses and disconfirming true ones. In statistics, these are referred to as Type I and Type II errors—false positives and false negatives. Much of the apparatus of statistical theory is devoted to techniques for avoiding these errors in ways that do not make finding "true positives" and "true negatives" too unlikely.

According to Franklin, during the Renaissance there was almost no development in legal theory, but there was widespread familiarity with medieval concepts of evidence and probability, including non-numerical grades of proof. These essentially Roman concepts were promulgated by two standard texts on legal evidence, Menochio (1587-90) and Mascardi (1584-88). Menochio held that innovations in law, particularly attempts to invent new *kinds* of evidence, are generally a sign of fraud (Franklin 2001, 45). This seems to have an obvious correlate in Hume's approach to the evidence for miracles as violations of the laws of nature—if someone is trying to prove something entirely novel (that is, against a perfect regularity of experience), Hume thinks we should distrust them simply because of what they are trying to prove.

It is worth making special note that the treatment of presumptions in Scots law is based on Menochio (Franklin 2001, 44). As I will show below, Hume was no doubt familiar with this, and it would be a short step from Menochio to the position that claims about violations of laws of nature are also likely to be frauds, even independent of the fact that fraud is known to be common with regard to purported miracles. At the very least, Hume's treatment of miracles is consistent with a long and careful tradition regarding evidential probability, one still very current in Hume's time, and beyond.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Although the development of the mathematical theory of probability was deeply influenced by the legal treatment of evidence, "By 1700 law had served its purpose for the mathematical theory of probability. The service has never been returned. Legal probability has continued to exist, and it is accepted in legal theory that such notions as proof beyond reasonable doubt involve probability. But all attempts to quantify the concept have been resisted" (Franklin 2001, 365). Even though the concept of "proof beyond reasonable doubt" appeared in English law around 1770, it was never treated numerically; Franklin (2001, 366) cites deliberately non-mathematical explanations of that concept from as recently as 1981.

Several reviews of Franklin's book suggest that there have been some attempts to apply mathematical probability to legal questions. Roberts and Aitken (2014, 105) point out, however, that "the English Court of Appeal has generally been hostile to any mention of Bayesian reasoning in criminal trials in England and Wales"; the courts "condemn any attempt to encourage jurors to employ formal mathematical models when evaluating evidence presented at trial. This is entirely consistent with orthodox legal theory stipulating that jurors should arrive at their verdicts using their ordinary common sense reasoning: it is precisely their ordinary common sense, untainted by specialist knowledge, which qualifies jurors as 'expert' decision-makers on the common law model" (Roberts and Aitken 2014, 105–106). The situation is similar in American law. Haack (2014) points out that while some legal scholars in recent decades have attempted to provide a mathematical interpretation of legal concepts of probability, courts so far have disagreed with this approach. Moreover, she argues persuasively that "legal degrees of proof are best construed as degrees of rational credibility or warrant and . . . degrees of rational credibility or warrant cannot be identified with mathematical probabilities" (Haack 2014, 64).

HUME AND THE ROMAN LEGAL TRADITION

So, to what extent is Hume influenced by the tradition of legal theory going back to the Romans? To my knowledge, Hume himself never asserts this connection; to my mind, that simply indicates that the tradition was so much a part of the fabric of the thought of the times that he did not feel it necessary to make it explicit. But besides the direct conceptual correlations discussed here, we have some circumstantial evidence as well.

In 1752, Hume was elected Keeper of the Advocates' Library, the largest and best library in Scotland and the third or fourth best library in the whole of Britain at the time (Michael Harris 1966, 92). Being elected a law librarian would seem to indicate that Hume was regarded by Edinburgh's legal professionals as having at least some knowledge of the law,

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

or at least that he would have acquired it through his duties there. However, "Of Miracles" was first published in the 1748 edition of the *Enquiry concerning Human Understanding*, and Hume's thoughts on miracles existed in some form when he was drafting the *Treatise of Human Nature*, which was first published in 1739-40, as his letter to his cousin in 1737 mentions (quoted in chapter 1). Hume's time as a law librarian—during which he was writing his *History of England*—was not where he had his first exposure to the Roman tradition of legal evidence.

So, where does Hume get exposure to the ancient Roman legal tradition? The answer, familiar to those who know Hume's biography, is that after Hume ceased his undergraduate studies (without taking a degree, as was then the common practice according to James Harris [2015, 35]), he returned to the university to study law. It didn't stick. He later said he saw a career in law as a lifelong drudgery that would have prevented him from pursuing the other sorts of studies in which he was interested (see Harris 2015, 35). But in the three years he was studying law, 1726–1729, he read legal textbooks and attended law lectures (Harris 2015, 35). "For the most part, Hume was unimpressed with the teaching he received at Edinburgh, but, in addition to more Latin and an introduction to Greek, he was exposed there to the culture of experimental natural philosophy, and to modern, Protestant, natural jurisprudence. Both made a lasting impression" (Harris 2015, 37).

There is little direct evidence of what, exactly, Hume was reading in this period as a law student. In *My Own Life*, Hume half-jokingly says that when his family supposed him to be reading Voet and Vinnius, he was instead reading Cicero and Virgil. As Harris notes, "Hume was probably required to read Johannes Voet's edition of the *Pandects* and Arnoldus Vinnius's edition of the *Institutes*. . . . Vinnius's edition of the *Institutes* [a manual for working lawyers] was explicitly aimed at young students of law" and Voet's edition of "the *Pandects*, or *Digest*, comprised a comprehensive account, in fifty books, of the laws of Rome" (Harris 2015, 41). An indication that Hume actually read these works, despite his joke to the contrary, is that when his private library was sold after his death, it contained the 1731 edition of Vinnius (see Harris 2015, 482, n. 27). In other words, Hume had to have purchased that book after ceasing his law studies in 1729, which he seems unlikely to have done without intending to read the book. Given that "The *Pandects* (Voet's edition) and the *Institutes* (Vinnius's edition) constituted two of the four elements of the compilation of civil law ordered by Justinian in the sixth century AD, and as such were fundamental to legal practice in jurisdictions, such as Holland and Scotland, which took their principles from Roman law" (Harris 2015, 41), it seems hardly plausible that Hume had not read or otherwise acquired familiarity with these foundational Roman legal texts in his time as a law student.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

We know, too, that the work of Hugo Grotius, a Dutch legal theorist and theologian, had a major influence on Hume. Grotius, founder of the school of natural law, had a remarkable influence on Scots culture into the 1720s, and works of natural law were taken to be the best manuals of moral and political instruction. "The impact of modern natural law is clearly visible in the account of morality that Hume gave in Book III of the *Treatise*. In [the *Enquiry concerning the Principles of Morals*], Hume claimed that Grotius's account of the origins of justice was the same as his own" (Harris 2015, 42). Snow (1902) discusses this connection between Hume and Grotius in some detail. And besides his general influence on European law, Grotius was particularly influential in Scotland. Hume's own undergraduate lecturer, William Scott, in 1707 prepared an edition of Grotius's *De Jure Belli ac Pacis* (On the Law of War and Peace; 1625) for his students's use. Haakonssen (1990) gives even more detail about the influence of Dutch natural law theory on Scots philosophy and of Grotius on Hume.

Whatever we should say about its direct influence on Hume specifically, it is true that legal theory had a huge impact on thinking about evidential probability generally. In the medieval and early modern periods, the law was "a model for reasoning in all those areas [including medicine, philosophy, business, politics, and so on] in which there is necessarily a balancing of opinions. This helps explain why the originators of mathematical probability were all either professional lawyers (Fermat, Huygens, de Witt, Leibniz) or at least the sons of lawyers (Cardano, Pascal)" (Franklin 2001, 350).

And, since Bacon was a lawyer, the fact that his account of evidential probability in science mirrors the treatment of evidence in the law is no surprise once the connections have been drawn. Hume's father, too, by the way, was a lawyer, and so was Hume's maternal grandfather (Harris 2015).

Like Franklin, Datson shows that the Roman legal tradition had a significant influence on the early development of the mathematical theory of probability.

> First, early probabilists . . . drew upon legal doctrines concerning aleatory contracts—that is, those involving some element of chance, such as games of chance and annuities—as sources not only of problems but also of fundamental concepts and definitions. . . . Second, legal theories of evidence supplied probabilists with a model for ordered and even roughly quantified degrees of subjective probability. The hierarchy of proofs within Roman and canon law led mathematicians to conceive of degrees of probability as degrees of certainty along a graduated spectrum of belief, ranging from total ignorance or uncertainty to firm conviction or "moral" certainty. (Datson 1988, 14)

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

The fact that Hume also speaks of a graduated spectrum of belief shows not that Hume was influenced by the mathematical theory of probability, but that both were influenced by the legal tradition.

Hume's "Of Miracles" is replete with direct and indirect references to legal concepts. Perhaps this is simply an ordinary trope to be found anywhere someone is trying to choose sides in a controversy. The adversarial method is as common in philosophy as it is in law, I suppose, and gathering and critiquing evidence, presenting a case, considering the counter-case, and answering it, is as much a part of ordinary reasoning as it is legal reasoning. Hume does, however, make explicit reference to "full proof," which is a concept from medieval law meaning proof sufficient for conviction in a capital case. He compares internal and external evidence—which are, again, medieval legal concepts. He mentions principles and concepts related to the weighing of evidence such as that a weaker evidence can never destroy a stronger, that more unusual claims require more evidence, the contrariety of evidence on either side, and competing witnesses. His account of the factors affecting witness credibility (EHU 10.7) is more or less directly borrowed from the medieval and early modern legal tradition. And he explicitly uses analogies of courts and judges deciding a case—he even mentions a legal example of a judge comparing the competing testimonies of two pairs of witnesses and deciding that the evidence on each side cancels each other out, leaving no basis for conviction (EHU 10.24). By the way, in the medieval European legal tradition that developed out of Roman law, "the corroborative testimony of two unimpeachable eyewitnesses constituted *complete proof* [sufficient for conviction in capital cases]" (Datson 1988, 42; emphasis added for clarity), which is why the example involves *pairs* of opposing witnesses rather than two *individual* opposing witnesses. There is even a suggestion in Hume (EHU 10.32) of something we today associate with legal statutes of limitations: it is difficult enough for courts to reasonably establish recent facts, and nearly impossible with regard to facts in the distant past.

Datson recognizes some elements of the influence of the Roman legal tradition on Hume's argument against miracles, for example, when she writes, "proceeding in legal fashion from probabilities to 'full proofs,' Hume argued that the intrinsic probability of a miracle—from 'the nature of the fact'—was zero" (Datson 1988, 325). Datson unfortunately also anticipates Earman's error in interpreting Hume here, though she is correct in seeing a legal influence in Hume's argument. In the passage she quotes to back up her claim, Hume says merely that "here is a direct and full *proof*, from the nature of the fact, against the existence of any miracle; nor can such a proof be destroyed, or the miracle rendered credible, but by an opposite proof, which is superior" (EHU 10.12). Since, for Hume, a proof is a category of probability and not a certainty of the sort we have in the case of relations of ideas, Hume cannot properly be interpreted

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

here to mean that there is *zero* chance that miracles can happen. In fact, in the clause following the semicolon, Hume lays out the conditions under which the miracle report *would* be credible (namely, when the evidence supporting it is superior to the evidence against it). Obviously (I hope), something that is logically impossible can never be made logically possible by the discovery of new evidence. The fact that Hume says it is possible for there to be evidence sufficient to credibly establish the occurrence of a miracle should by itself, then, tell us that he does not mean that he has proven that the occurrence of miracles is impossible. A *proof against the existence of any miracle* is still an *epistemic* rather than *ontological* claim for Hume, because *proof* is an epistemic category. Unfortunately, Datson's subsequent discussion of Price's rebuttal of Hume on miracles perpetuates Price's error of thinking that Hume had claimed to have shown the impossibility of the occurrence of miracles.

Hume's family connections to the law, his legal training, his time as a law librarian, and the direct references to legal concepts and legal methods of reasoning about evidence in "Of Miracles" are all circumstantial evidence supporting the claim that Hume was deeply influenced by the evidential tradition going back to ancient Roman and medieval law.

Legal theory is not the only possible direct influence on Hume, of course. Similar sentiments are to be found, for example, in Montaigne (who is also, by the way, a lawyer). Montaigne comments on reports of flying witches: "How much more natural and likely it seems to me that two men [the legally required pair of witnesses] are lying, than that one man should pass with the winds in twelve hours from the east to the west" (Montaigne 2003 [1588] *Essays* 3, chapter 11, 961). Montaigne also comes very close to Hume's position on miracles with this: "It seems to me we may be pardoned for disbelieving a marvel [miracle], at least as long as we can turn aside and avoid the supernatural explanation by nonmarvelous [nonmiraculous] means" (*Essays* 3, 11, 961).

We know, too, that Hume was influenced by both the style and content of Cicero's oratory:

> The classical rhetorical tradition that derived from Aristotle and Cicero was also a qualitative one. For Aristotle, probabilities (endoxa; "resting on opinion") concern that which generally but not invariably happens. . . . The Roman and medieval rhetoricians followed suit, parroting the classical definitions. Nor should the ordered stages of certainty—moral, physical, and metaphysical—of Glanvill, Wilkins, Boyle, and other early Royal Society luminaries be confused with a full continuum of degrees. . . . Rather, they regard the various levels (particularly moral certainty) as thresholds of belief, and as an expansion of Aristotle's view that not every subject admits the same degree or kind of certainty. (Datson 1988, 38-39)

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Of course, the classical rhetorical tradition also influenced and was influenced by the classical legal tradition. Cicero, too, was a lawyer.

Wootton (1990) points out that Nicholas Fréret, author of "Reflections on the study of ancient histories and on the degree of certitude of their proofs" (1729), provides a treatment of the evidence for miracles that is very much in line with Hume's approach. That work could be a source for Hume's argument, according to Wootton. While there is no direct evidence that Hume read Fréret, the work in question "discussed not only testimony in general, but the testimony in support of prodigies and miracles in particular. . . . This is not surprising, since he, like Hume, was dealing with a standard set of problems in the theory of 'degrees of assent,' a topic which he saw as an aspect of the theory of probability" (Wootton, 202). Wootton follows Hacking (1975) in thinking The Port Royal Logic presents an early stage of the mathematization of probability and that everything after that was leading up to our contemporary conception of probability as mathematical, so his parenthetic remark immediately following the passage just quoted is meant to be a criticism of Fréret: "(Indeed he [Fréret] was keen to show off his knowledge of the work of the Bernoullis and of other mathematical theorists of probability, while dismissing it as irrelevant to the immediate issue [of the evidential probability of miracles])." But from the point of view of the account of non-mathematical probability offered here, this is an especially juicy hint. Here is a possible source for Hume's argument against miracles who fully appreciates the then-developing mathematical theory of chances, and who nevertheless rejects that approach in the context of the evidential probability of reports of miracles. Fréret, by the way, was the son of a magistrate and, briefly, himself a lawyer before turning to intellectual pursuits, mostly in the study of history.

We may add that Locke's theory of probability, which clearly was an important influence on Hume, is "more or less identical to the old [Roman] legal one" (Franklin 2001, 371).

My point here is not to show specific influences on Hume (for a discussion of Hume's possible sources for "Of Miracles," see Wootton 1990). Rather, my point is to make the case that these parallels indicate that Hume's treatment of miracles is consistent with a long and careful *non-mathematical* tradition regarding evidence and probability, one that was still current at the time Hume was writing. If that old tradition is incorrect, it is certainly misguided to lay the blame for its incorrectness entirely at Hume's feet.

RESPONDING TO A POSSIBLE OBJECTION

A possible objection to the account being sketched here is that there is really only one correct conception of probability, the Pascalian one. On

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

that conception, probability is supposed to be about the *balance of reasons* for or against some claim. But an additional consideration, not fully captured in that way of thinking about probability, has to do with the difference Suarez points out between negative doubt (no reasons for or against) and positive doubt (a balance of reasons against; see Franklin 2001, 77). Thus, the number expressing the balance of reasons is not the whole story about evidence. Keynes (1921, 71–78) raised this issue under the heading "weight of evidence" in his *Treatise on Probability*. On this account, Pascalian probability is about the balance of reasons, whereas Baconian "probability" is just a confused attempt to get at whether there are any reasons. They are not two separate accounts of probability, they are talking about two entirely different things. (I thank James Franklin for raising this potential objection in personal correspondence, in which he also points out that the issue of how to deal with weight of evidence is as unresolved as ever in the current Bayesian bible [Jaynes 2003].) Hume agrees with the point here that evaluating probabilities is a matter of weighing the evidence for and against. Even so, he would say that we never have zero or 100 percent probability with regard to empirical propositions. The contention in the objection just described thus does not seem to me to be correct, for the following sorts of reasons.

A key point in Hume's epistemology is that the contrary of any contingent proposition (matter of fact) is possible. In Pascalian terms, that is to say that disproof does not amount to 0 percent probability, and proof does not amount to 100 percent probability. Lack of proof one way or the other, or an equal balance of probabilities on both sides, does not for Hume give us the 50 percent probability that the (Pascalian) principle of indifference would give us. Rather, in such a situation Hume would say that we ought not to believe either proposition. We would then have perfect suspension of belief, as in the case of contrary miracles. Accordingly, I do not think it is correct to characterize Pascalian probability as being about the balance of reasons and Baconian probability as being about whether there are any reasons. Pascalian probability can very well assess *reasons for* without considering *reasons against*, or without comparing contrary propositions. Likewise, Hume is very much concerned with the balance of probabilities—in fact, the argument against believing in miracles depends on there being a huge over-balance of evidence for the law that has supposedly been violated. When the balance of probabilities on either side is exact, we have *lack of proof* one way or another (not *disproof*, notice). When there is an exceptionless regularity of past experience, we have what amounts to a full proof.

Thus, in Hume's view at least, there really are two species of probability, mathematical and non-mathematical, that apply respectively to situations in which we have complete knowledge of the stochastic setup (the probability of chances) and to situations in which we reason based on the available evidence about whether or not a given proposition is worthy of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

assent (the probability of empirical belief). The two are not entirely independent since, for example, it makes sense to say that our degree of belief about the outcome of the roll of a die should be governed by what the stochastic setup tells us.

CORRECTING HACKING'S HISTORY

Near the beginning of the *Abstract* of the *Treatise of Human Nature*, which Hume wrote to try to drum up interest in his work and published anonymously, Hume writes that,

> The celebrated *Monsieur Leibnitz* has observed it to be a defect in the common systems of logic, that they are very copious when they explain the operations of the understanding in the forming of demonstrations, but are too concise when they treat of probabilities, and those other measures of evidence on which life and action entirely depend, and which are our guides even in most of our philosophical speculations. . . . The author of the *Treatise of Human Nature* seems to have been sensible of this defect in these philosophers, and has endeavoured, as much as he can, to supply it. (Hume 2000, *Abstract* 4, 408)

Hume's self-assessment here seems to me to be accurate. Probability does guide (is the main inference type utilized in) most of the most important and most philosophically interesting parts of philosophy and of ordinary life. And Hume does spend significant time on probable reason—both in THN and EHU. Unfortunately, commentators have not always seen clearly what Hume meant in his account of probability. This strikes me not as a result of Hume's remarks on probability being opaque—they are not—but rather as a result of other external reasons. One is the timing of Hume's publication of the *Treatise*, at an inflection point in intellectual history leading to intense interest in mathematical accounts of probability, largely due to the rise of science. Then, by an accident of historical scholarship, many recent commentators on Hume's account of probability have not had Hume as their main focus but instead have commented on him in passing or as an afterthought while writing about other topics in the history of philosophy and the history of probability. (I'm thinking here, for example, of seminal works by Datson, Hacking, and even Franklin, among others.) Another contributing factor, I think, is that, perhaps partly due to the rise of Logical Empiricism in the early twentieth century and the attendant shift toward symbolic and mathematical analysis in philosophy of science and related subfields, some historians of philosophy have been reluctant to challenge mathematical analyses of historical figures. It is a field in which they often feel ill-equipped (many of us went into philosophy partly because we liked depth and rigor but not math). Some historians of philosophy also, I think, see through the pretensions of mathematical analyses of historical figures but do not want to spend

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

their time writing about it, hoping in part that ignoring it will make it go away. It is also true that historians of early modern philosophy do not typically read works of ancient and medieval legal theory. Finally, so many commentators have made so many disparate claims about Hume's account of probability that his views have become muddled to many readers today.

As one example of one of these accounts that has muddied the waters, consider Ian Hacking's publications on probability theory and its history. They have been very influential, and although I have great respect for Hacking's body of work, in my opinion the impact of his "Hume's Species of Probability" (1978) has been quite unfortunate since in it (in my view) Hacking entirely misrepresents Hume's thoughts about probability. One of my aims in this section is to correct Hacking's errors and set the record straight. I also take the opportunity to go deeper into some aspects of the account of Hume's approach to evidential probability that I have been developing in this book.

Hacking explores three themes relating to chance and probability in Hume's *Treatise of Human Nature*. Those themes are:

1. That careful attention to the chapters of Book 1 on chance and probability are necessary to properly understanding Hume's account of cause as constant conjunction, which is absurd without them.
2. "[T]his study of probability matters to the history of ideas because Hume raises a problem about knowledge which could not have been put before that time, but which has vexed epistemologists ever since" (Hacking 1978, 21).
3. "Hume was on the verge of saying something he ought to have said but which has for more than two centuries gone unsaid" (Hacking 1978, 21).

While I agree with Hacking's position on (1), I am less than enthusiastic about (2) and (3). The importance for the history of ideas of careful study of Hume's remarks on probability has do instead with the fact that Hume's account of probability is not equivalent to or compatible with the mathematical theory of probability that Hacking sees Hume as anticipating.

Hacking develops his account by looking at how what Hume says about knowledge and probability is different from the stereotypical view of early moderns such as Descartes who employ an ancient distinction according to which *scientia* and *episteme* refer to knowledge that is certain (on the model of Euclidean geometrical reasoning) while *opinio* can come by degrees but is not "knowledge" in the true sense of the word. What Hacking unfortunately neglects is that Hume and his actual historical antecedents do not draw the distinctions in this stereotypical way. Since

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Hacking does not understand the history behind Hume's views on probability, he does not know that they are connected to a very old tradition.

Hacking has similarly been criticized for remarks in his book *The Emergence of Probability* (1975) that get the historical details slightly wrong. For example, Robert Brown (1987) shows that Hacking's *The Emergence of Probability* (1975) is incorrect in its assertion that there was no concept of inductive evidence before the seventeenth century, that Hume was the first person to formulate the skeptical problem of induction (Hacking's theme [2] listed above), and that the "concept of the internal evidence of things is primarily a legacy of the low sciences, alchemy, geology, astrology and especially medicine" (Hacking 1975, 35). Brown illustrates clearly that there were medieval and ancient philosophical antecedents and even explicit statements of the first two, and that the high sciences in the medieval period shared a concept of internal evidence with the low sciences (at least for some writers in the medieval period). In short, Hacking is wrong in his history of the rise of the concept of evidential probability. Both Brown and Hacking neglect the *legal* background that Franklin (2001) shows is so crucial. Standard legal thinking about probable evidence is actually the model for thinking about probability in philosophy and in the sciences throughout the medieval and early modern periods.

AVOIDING THE PROBLEM OF THE PROBABILITY OF UNIVERSAL GENERALIZATIONS ALWAYS BEING ZERO

One major advantage of Hume's actual account of evidential probability, as compared to some "straw" versions of his account, is that he does not have to worry about falling into the position Popper and other advocates of Pascalian probability are locked into, namely being forced to accept that every universal empirical generalization has a probability of zero—that is, that there is no possibility of an empirical generalization being true. (Compare Hacking 1978, 35.) Pascalians are forced to this conclusion because anything that is not a logical truth has possible exceptions. Thus empirical generalizations have exceptions (known or unknown) and are not universally true, and propositions known to be false have a probability of zero in the Pascalian scheme.

Hume, instead, can say that we have evidence amounting to a *proof* for many of our ordinary and scientific generalizations: They are not guaranteed to hold true in absolutely every future case, but no reasonable person would doubt that the generalizations will in fact hold. Moreover, Hume can have it that different generalizations, which have obtained with different degrees of regularity in the past, beget accordingly stronger and weaker expectations of future uniformity. Both of these features of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Hume's account are consistent with what we actually do when we reason about evidence.

In fact, I would consider these behaviors to be cognitive phenomena that need to be accounted for by any adequate theory of evidential probability. The failure of the basic Pascalian theory to account for them, or the contortions Popper and other Pascalians have to go through in order to avoid the failure, is reason enough to doubt the Pascalian approach as a general account of evidential probability.

As Hacking remarks, it may be that the problem just identified is simply "specious and arises . . . from conflating the two species of probability. Only prejudice formed by two centuries of unreflective use of numbers makes us equate the two different kinds of situation" (1978, 35). I heartily agree with Hacking on this point, though I do think he has mischaracterized just what the two species of probability are. He cashes out the distinction this way:

> There is the case in which all the evidence points all the same way, towards an universal generalization, and only caution for awhile pulls us short of complete confidence. Then there is another situation in which we know the universal generalization is false because we have occasional counterexamples. The latter leads me to a judgement of probability that is simply different from the kind of probability that can attach to an exceptionless universal generalization. It is sheer confusion to try to compute the latter by a scheme that has been devised for the former. (Hacking 1978, 35)

This is possibly correct if Hacking is making a claim about empirical psychology (although I doubt that the claim is correct even then). Hacking is, however, dead wrong to attribute this way of drawing the distinction to Hume. As I have shown in previous chapters, Hume's distinction is between certainty and probability—where probability *includes* his category "proof" (moral certainty) as its peak. Exceptionless past experience gives us evidence amounting to a proof that the observed regularity will hold universally in the future, and in such cases we *act as if* the regularity were truly certain, but with the caveat that we know that it is not truly certain. Thus, the kinds of cases Hacking distinguishes in the above passage are in fact all on the same scale for Hume—they differ only by degree, not by kind, contrary to Hacking's claims in the passage quoted.

HUME'S SPECIES OF PROBABILITY

So, then, what is really going on in Hume? Will Hacking's overall conclusions hold up when Hume is properly understood? Hume's actual distinction between species of probability is in fact threefold: probability of chances, probability of causes, and analogical probability. Dorothy Coleman has argued that,

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> Butler's argument and its reincarnation in Hambourger [mentioned in chapter 3, above; see also Butler 2006] overlook two senses of probability: probability pertaining to events qua unique occurrences and probability pertaining to events qua instances or tokens of event types. This distinction approximates that made by Hume in the *Treatise* between "probability of chances" and "probability of causes" [THN 1.3.12 and 13]. Hume's argument against the believability of miracles invokes the second sense, whereas the Butler/Hambourger argument invokes the first. Following the first sense of probability, the likelihood of an event is measured by its degree of predictability as a unique occurrence; following the second sense, it is measured by its degree of conformity to causal laws applicable to events of its type. (Coleman 1988, 333–334)

In later work, Coleman emphasizes that Hume actually employs a third species of probability as well. He employs it prominently, namely in Section 9 of the *Enquiry*, "Of the reason of animals," which immediately precedes and sets up the argument against miracles in Section 10. Coleman quotes the first paragraph of that section, where Hume says that,

> All our reasonings concerning matter of fact are founded on a species of Analogy, which leads us to expect from any cause the same events, which we have observed to result from similar causes. Where the causes are entirely similar, the analogy is perfect, and the inference, drawn from it, is regarded as certain and conclusive. . . . But where the objects have not so exact a similarity, the analogy is less perfect, and the inference is less conclusive; though still it has some force, in proportion to the degree of similarity and resemblance. (EHU 9.1)

Coleman points out that analogical probability provides another way to understand Hume's view that some contrary proofs can have more weight than others, despite a proof being a state of belief in which we have practical certainty:

> Proofs, probability of chances, and probability of causes presuppose judgments about similarities and differences among evidential instances. The weight of these proofs and probabilities of chance and causes may vary in light of the strength of their analogical evidence. . . . One proof can be stronger than another if it is based on greater relevant resemblances than another. Whereas probability of causes measures quantitative variations in rates of occurrence, analogical probability measures the weight or force of this evidence. (Coleman 2001, 204)

By way of a concluding aside for this section, let us note that it is tempting to think that Hume does not need the modifier "evidential" in the phrase "evidential probability" because for him all probability is assumed to be evidential, in the sense that it concerns degree of belief concerning matters of fact. We could then explain his phrase "the probability of chances" as a way to distinguish ordinary (evidential) probability from the aleatory concept. The problem with this understanding, unfortunately, is that for Hume degree of probability is really just degree of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

belief, cashed out as force and vivacity, and there are other psychological mechanisms besides evidential reasoning that affect force and vivacity. This means it will still be useful to use the phrase "evidential probability" to distinguish the reasoned formation of beliefs about matters of fact from other ways of arriving at those beliefs.

HACKING ON HUME ON GRADES OF PROBABILITY

"Hume wrote when for the first time there is a continuum or the threat of one between what he calls probability and proof, or between what we should call grades of belief and the certainty of knowledge" (Hacking 1978, 22). I do not care to comment here on whether or not there was in Hume's time the beginning of a view that there exists a continuum between probable belief and certain knowledge. I do wish to point out that Hume holds no such position. Hume's distinction between matters of fact and relations of ideas is strict: Only relations of ideas can be known with certainty; matters of fact can be known only with some degree of probability that is always short of demonstrative certainty even in the best cases. Unfortunately, Hacking is not alone in misreading Hume in this way. In part, it seems to me, the source of the error is misunderstanding what Hume means by "proof." Besides correcting our account of the history of ideas, it is important to make clear Hume's view because he offers an account of evidential probability that shows promise of being superior to the Pascalian one that is dominant today.

> Now Hume's usage, which is not far from ours, is a sharp break from the traditional theory of knowledge, which kept knowledge—*scientia* and *episteme*—quite separate from the *opinio* which came in degrees of probability. There could be no transition from probability to proof, in the old scheme of things, because probability applies to a quite different category of object than did knowledge. (Hacking 1978, 22)

In fact, as I argue, Hume's usage is *not* ours, not even close. Hacking makes errors about Hume's theory of probability and about his epistemology generally. To a large extent, Hacking's errors arise from his reading Hume too much through the lens of the mathematical approach to probability. Hume strictly distinguishes *matters of fact* and *relations of ideas* and, in his epistemology, never the twain shall meet. The transition from probability to proof is unremarkable in Hume except that we have thereby achieved the zenith of evidence and belief with regard to an empirical matter of fact. The transition from probability to *demonstration* never happens according to Hume since the two categories apply to different kinds of propositions.

In a similar vein Hacking contends that "Hume is the first philosophical settler in the terrain of probability which had been sighted only 80 years before, in the 1660's" (Hacking 1978, 22–3). I deny this outright.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Hume is aware of the developments in the Pascalian theory of mathematical probability, as Dorothy Coleman has shown. Coleman argues that Hume's discussion of the probability of chances (as opposed to the probability of causes and analogical probability) "shows without controversy that he was familiar with the basic concepts of probability based on the calculus of chances" (Coleman 2001, 201; see THN 1.3.11 and 1.3.12, and EHU 6). Hacking certainly agrees, since he runs together Hume's positions on probability under the mathematical heading. But Hume, as I have argued, has an altogether different view of evidential probability, one incommensurable with the Pascalian view. To say that Hume is the first philosophical settler in the territory of mathematical probability is simply incorrect; Hume does not live there—he has barely even visited.

In Hacking's description of what he calls Hume's first species of probability or "probabilities connected with associations that have in fact been entirely uniform in experience" (1978, 27-8), it becomes clear that Hacking reads "proof" for Hume to mean something that is perfectly certain, whose probability is 1. Hacking quotes (THN 1.3.13.3), where Hume writes that "the gradation from probabilities to proofs is in many cases insensible." Hacking unfortunately reads Hume here as saying that we stop short of "absolute certainty" in our reasonings concerning matters of fact *because* "we have learned that generalizations are beset by counterexamples" (Hacking 1978, 28). For Hume, rather, we never achieve absolute certainty regarding matters of fact simply because of the kind of knowledge in question: Matters of fact are not relations of ideas. We *can* however achieve *proof* or *moral certainty*, and end up with well-grounded expectations about past uniformities continuing into the future.

Hacking goes on to give some examples involving the imperfectly uniform purgative powers of rhubarb, but the examples seem either to endorse the straight rule of induction or saddle Hume with it. As argued earlier, there is no acceptable interpretation of Hume on which he adopts the naïve straight rule of induction.

Hacking complains (1978, 28, *et passim*) that Hume analyzes the probability of causes in terms of the probability of chances, when the two should be treated separately:

> It is simply wrong to conflate the two species of probability. . . . I agree that in practical cases it may make little difference whether my doubt is brought about by knowledge of actual contrary causes, or is simply a modest and abiding skepticism. But they are different doubts and there is no *prima facie* reason to suppose that their mathematical explication should be the same. . . . Hume set the stage and ever since we have been conflating the two species of probability. (Hacking 1978, 32)

I think Hacking is correct to insist that these really are two separate species of probability, and that there is no good reason to treat them in the same terms. In fact, against Hacking, I think one of them ought not to

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

be explicated in mathematical terms at all. But never mind what I think: Hume says merely that the two are "like species" (EHU 6.1) of probability or belief—that is, he asserts only an analogy, not an identity. And, as mentioned above, Hume does not in fact conflate these two kinds of probability. This fact is difficult to see if one reads Hume's remarks on evidential probability through the lens of the mathematical theory of probability. Hume's position is that events subject to the probability of chances can be known in advance only to some (mathematical) degree of probability that is determined by the chance setup (which itself might or might not be fully known), whereas the activity of contrary causes produces probable belief (degree of vivacity) in an entirely different way.

Hacking quotes Hume as writing that proofs may degenerate insensibly into probabilities (Hacking 1978, 22, referring to THU 1.3.13.3). Besides misattributing a mathematical conception of probability to Hume here and thinking that Humean "proof" means a probability of 1, Hacking also misses the crucial point that this comment of Hume's about proofs degenerating into probabilities appears in the section "Of unphilosophical probability," wherein Hume is concerned to analyze typical cases of *bad* evidential reasoning—paradigmatic fallacies of reasoning about matters of fact, if you will. These mistakes, including the mistake of letting a proof degenerate insensibly into a mere probability by "the multitude of connected arguments," have a purely psychological source. In this case, the human mind is not very good at keeping track, and the degree of vivacity of the original impression quickly diminishes as the number of stages of inference increases. Since Hume has it that degree of belief is vivacity of belief, the degeneration of proof into probability in such circumstances is a feature of human cognitive psychology that is unavoidable even if not epistemically ideal. Epistemically speaking, proof *should not* degenerate into (mere) probability, but because of unfortunate features of human cognitive psychology, it sometimes does.

Characterizing what he takes to be Hume's view, Hacking comments that, "There could be no transition from probability to proof, in the old scheme of things, because probability applied to a quite different category of object than did knowledge. . . . [B]y the time of Hume, knowledge was being carved up in an entirely new way, which is still with us. For the first time probable belief could, through the accumulation of evidence, become an item of knowledge" (Hacking 1978, 22). Again, I think this simply gets Hume wrong, even putting aside the issue of what was happening in general in the field of epistemology in Hume's time. Hume is not Locke, of course, and does not insist that only things known with certainty can be termed science or knowledge. But then again, Locke himself does not discount the epistemic force of merely probable beliefs—they cannot be called *scientia*, but they are still sufficient to sway belief and govern thought and action.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

It is worth noting here that THN 1.4.1.1 also contains a nice statement and application of Hume's treatment of interfering causes. Often, Hume says in general, causes appear to act irregularly because of interfering causes—that is, the causal situation is really governed by regular laws, but some of the causal laws in play are (or may be) unknown to us:

> Our reason must be consider'd as a kind of cause, of which truth is the natural effect, but such-a-one as by the irruption of other causes, and by the inconstancy of our mental powers, may frequently be prevented. By this means all knowledge degenerates into probability; and this probability is greater or less, according to our experience of the veracity or deceitfulness of our understanding, and according to the simplicity or intricacy of the question. (THN 1.4.1.1)

Then at THN 1.4.1.5, Hume remarks that "Here then arises a new species of probability to correct and regulate the first, and fix its just standard and proportion." It is a new species of probability only in the sense of following after the first judgment we make (given the evidence) about some matter of fact as having some particular probability of being true. It is a new species of probability in that it has a different object than the probability at the level of the judgment of the probability of the truth of the matter of fact; the new species of probability is a meta-level judgment about the probability of the correctness of the first level probability judgment. Both are still what we would call judgments of evidential probability, so it is not new in that sense.

Hume's conception here is not very different from the use in statistics of a confidence interval and a confidence level. Hacking seems to misread Hume on this point, since he writes that, "Hume regularly confuses what we may call levels of probability. A probability of the first level arises from the evidence bearing directly on the case at issue. Second level probabilities concern the extent to which one can rely on inferences to probabilities at the first level" (Hacking 1978, 30). This comment is baffling in light of the passage at THN 1.4.1.5 mentioned above, where Hume is explicitly considering exactly these two levels. More baffling still, Hacking (1978, 30) claims that "The worst example of Hume's confusion of levels of probability is in Chapter I, Part IV, Book I, on scepticism with regard to reason"!

ἘΝΑΡΓΕΙΑ (ENARGEIA), EVIDENTNESS, AND EVIDENCE

Allen (2001, 1) says that "Cicero introduced *evidentia* as a rendering of ἐνάργεια (*enargeia*), the quality of being evident" or what in English we call self-evidence. The ancient Greeks discuss what we today call evidence, "an item that is the basis of an inference or the ground of a conclusion" (Allen 2001, 1), under the heading of "inference from signs." One sees the phrase "inference from signs" throughout the discussion of the

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

history of non-numerical probability in Franklin (2001), right up through the medieval period. Cicero, whom Hume greatly admired, is not the only interesting connection to the themes explored in this book. In his glossary of Greek rhetorical terms, Anderson (2000, 43) defines ἐνάργεια (*enargeia*) as "vividness": It is "the art of vivid expression, often described in terms of setting matters before the eyes of the audience." The earliest use of the word "vividness" in English was in Robert Boyle's 1668 discussion of light and colors; the vividness of *ideas* was first mentioned in print in 1768. Even the word "vivid" (from the Latin *vividus*: animated, lively) only made its first appearance in English in 1638.

Locke used the word "vivid" in 1690 (2.19.4, 112): "Those Motions made on the Organs of Sense, which at other times produce very vivid and sensible Ideas." Before that, the term we know so well from Hume, *vivacity*, was used in English beginning in 1475 to refer to quickness, liveliness, force or power—vivaciousness—both with regard to a person's general mental capacities and with regard to specific ideas. (See the respective entries in the Oxford English Dictionary for more detail.)

> The connection between the two senses [of "evidence," namely *being self-evident*, and *being a basis for an inference*] seems to be this: to serve as evidence for a conclusion, apart from supporting it, an item must be evident, or at least more evident than the conclusion. Only in this way can it permit us to infer a conclusion that we do not know from grounds that we do, thus adding to our stock of knowledge. (Allen 2001, 1)

As a set of connections, these themes of (a) setting before the senses, (b) making vivid, and (c) being a ground for an inference from seen to unseen are all clearly present in Hume's treatment of evidence, inference, and probability.

As Allen (2001, 2) notes, the idea of inference from signs is very old. Aristotle, for example, "remarks that it is necessary to use visible things as witnesses for the invisible." Allen traces the ancient Greek and Hellenistic debates over "signifying relations," noting that for some authors (for example, Augustine, and the medical Empiricists) there was very little "inference" between signifier and signified, sometimes no more than a mental association explained as a kind of remembering. "[T]his kind of sign-inference depends on a relation of association formed somehow in the memory of the person drawing the inference" (Allen 2001, 6). The similarity to Hume's principles of association of ideas is immediately obvious.

Aristotle may have been the first to distinguish signs (the merest part of evidence) and demonstrations (which explain at the same time that they justify a conclusion). In other work, Aristotle focuses on inferences from signs, and emphasizes "the contrast between evidence which yields a conclusive argument and evidence which only serves to make a conclu-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

sion probable or likely. He calls the latter signs, and the former tokens" (Allen 2001, 8). Though Aristotle's approach to the sign/token distinction was not much pursued by later writers, there are affinities to it in Hume's distinction between demonstrative and probable reasoning.

Aristotle's distinction between dialectic and rhetoric has to do in part with the nature of the issues that the two address. In rhetoric, "These issues are, as Aristotle puts it, matters that permit of being otherwise. . . . [T]here is an ineliminable roughness and inexactitude to them, which imposes correspondingly greater demands upon the faculty of deliberation" (Allen 2001, 18). Inference from signs is thus applicable in practical realms such as ethics, law, and politics, where demonstrative reasoning is mostly inapplicable but where we can draw conclusions that (as Aristotle puts it) are likely or which describe things that happen "for the most part."

As we have seen, Hume's position on knowledge is that certainty is possible for relations of ideas and deductive arguments, but that with regard to matters of fact it is possible only to have probable knowledge. Like just about anything else in philosophy, Hume's position has historical antecedents. In the ancient Greek world up through the Hellenistic milieu, there is a vigorous debate between dogmatic (rationalist) views according to which it is possible to acquire knowledge that is certain regarding the reality underlying appearances, on the one hand, versus skeptical views that deny the very possibility of such knowledge—or even the possibility of any knowledge at all—on the other.

As Hume seems to have understood it (as we will see, mistakenly), the Pyrrhonists held the most radical view in the skeptical camp, by his lights officially doubting all things, even knowledge derived directly from the senses. A more careful reading, however, shows that on Sextus Empiricus's version of Pyrrhonism at least, Pyrrhonists realized the necessity of *acting as if* the phenomena were real, *as if* laws of nature were real, and so on. The feeling of hunger, though perhaps an illusion, is nevertheless sufficient human motivation to eat bread *as if* one believed the empirical generalization that bread nourishes.

> [T]he Pyrrhonist, having put every thing on the same level and suspended judgement on every matter regarded as an object of philosophical enquiry.... finds himself left with views which largely correspond to ordinary, everyday views of common experience; these are the appearances he follows for lack of anything better, while remaining acutely conscious of the questions that can be raised about them. (Allen 2001, 107-08)

This, of course, sounds very much like Hume's distinction between the doubts appropriate in philosophical as opposed to ordinary contexts. (See Ribeiro 2009, 10 *et passim*.) Hume makes a direct reference to this in the *Treatise*:

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> But a PYRRHONIAN cannot expect, that his philosophy will have any constant influence on the mind: Or if it had, that its influence would be beneficial to society. On the contrary, he must acknowledge, if he will acknowledge any thing, that all human life must perish, were his principles universally and steadily to prevail. All discourse, all action would immediately cease; and men remain in a total lethargy, till the necessities of nature, unsatisfied, put an end to their miserable existence. (EHU 12.23)

For his part, Sextus Empiricus answers this "*apraxia*" ("inaction") objection this way:

> Thus, attending to what is apparent, we live in accordance with everyday observances, without holding opinions—for we are not able to be utterly inactive. These everyday observances seem to be fourfold, and to consist in guidance by nature, necessitation by feelings, handing down of laws and customs, and teachings of kinds of expertise. [...] And we say all this without holding any opinions. (*Outlines of Pyrrhonism* 1.23-24, as quoted by Ribeiro 2009, 9)

Ribeiro (2009) makes the case that Hume in effect himself adopts the *actual* Pyrrhonist view (as opposed to the caricature of the Pyrrhonist view that Hume dismisses). This is consistent with Allen's (2001) account of the Pyrrhonist view, too. Readers who wish to dive into this question in more detail will want to read Fosl (1998) as well, which examines the sources through which Hume came to know Sextus and the Pyrrhonists.

Reasonable, tentative, practical—these are the words that may best describe Hume's overall, considered, epistemological position. For Hume as for the real Pyrrhonists, philosophical doubts regarding ordinary subjects are of greatest significance when someone (perhaps oneself) purports to have certainty with regard to weighty claims that are in fact not certain. It does not matter much if one eats (the illusion of) bread expecting it to sate (the illusion of) personal hunger; it may matter a great deal that someone dogmatically attempts to impose a religious or political idea on the lives of others when it might not be correct.

According to Hume *we cannot help but assent* to straightforward empirical generalizations founded on long experience of a constant conjunction. It *is possible to doubt*, and *we should suspend judgment*, in cases where more is at stake and the evidence when carefully weighed and considered is inadequate to give us a sufficiently high degree of belief in the claim in question to ground reasonable belief and choice of action. Hume's account of a (non-rational) custom or habit of the mind according to which we expect present cases to resemble past cases is not at all dissimilar to Sextus Empiricus's view. Sextus holds that while indicative signs (which purport to illuminate real connections in nature) are bogus, commemorative signs (which merely remind us of connections we already have experienced) are, though not guaranteed to be true, accept-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

able for the most part. (See Allen 2001, 138ff.) Hume echoes this view almost exactly. "It is important to recognize that the mitigated skepticism of the *Enquiry* is not the result of reasoning. Reason, left to itself, provides no response to [radical skeptical] arguments. Mitigated skepticism results from the interaction of two forces: a *departure* from common instinctive belief, counteracted by the *persistence* of common instinctive belief" (Fogelin 2009, 158).

CONCLUSION OF CHAPTER 4

This chapter has described the legal tradition of reasoning about evidence and probability that derives from ancient and medieval sources. That tradition was live and flourishing in Hume's day, and as a former law student if for no other reason Hume would have been directly aware of it. Hume's remarks on probability fit exactly with the rules and concepts of this legal tradition of evidence, which was (and still is) entirely and deliberately non-mathematical. This tradition fits, furthermore, with what Cohen and Coleman call the "Baconian" conception of probability as contrasted against the numerical conception of probability they call "Pascalian." In the non-mathematical tradition, the lower bound of probability is "lack of proof" and the upper bound "practical lack of doubt." These concepts cannot be captured within the mathematical theory and certainly do not map to zero and 100 percent probability, respectively. Given the incommensurability of these two paradigms of probability, it is a mistake to attempt to interpret Hume's argument against miracles through the lens of the mathematical theory of probability.

In this chapter I have argued, too, that Hacking's account of Hume's species of probability, though influential, is fundamentally flawed. A careful reading of Hume's comments on probability is important to understanding his overall system, including his views on causation and induction. Such a careful reading and attention to historical context shows, however, that Hume was not a Pascalian about evidential probability, and that he was not at all the first to express doubts about the foundations of inductive reasoning. Antecedents of Hume's general mitigated skepticism, as well as of particularities of his views on probability and knowledge, can be found in ancient and medieval sources.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

FIVE
Hume and the Bayesians

IS HUME'S MAXIM A DEMARCATION CRITERION?

One of Earman's themes is that Hume's theory of induction is flawed and is a source of error in Hume's argument against miracles (Earman 2000, especially 9–26 and 29–32). It is true that Hume's argument against believing testimony about certain kinds of events depends on his theory of the inductive generation of universal empirical beliefs (laws of nature), as I showed earlier. And Earman may be right that Hume's theory of induction is somewhat primitive and incomplete—it is, if nothing else, based on a theory of ideas that many contemporary epistemologists take to be untenable. Nevertheless, I contend that Earman's attack on Hume fails, and that it fails because he misunderstands key elements of Hume's epistemology. There may well be problems with Hume's argument against miracles, but they are not the problems Earman identifies.

By now the righteous indignation of the community of Hume scholars at the opprobrium heaped upon Hume by Earman has been sufficiently recorded. Many reviews of Earman, and even reviews of Fogelin (2003), have remarked on the excessive invective in Earman's account of Hume. (See, for example, Gaskin 2003, Levine 2002, McGrew 2005, and Otte 2005.) The point here is not to again wag a finger at Earman for attacking our beloved Hume, but simply to show some of the instances of Earman's interpretive errors to correct the record and in the hope that others will not commit the same mistakes.

Earman claims that Hume's argument is an example of the kind of over-reaching that gives philosophy a bad name (Earman 2000, 3). He compares Hume's maxim against belief in miracles to the Logical Positivists's attempts to devise a "demarcation criterion" that would define necessary and sufficient conditions for classifying some kinds of knowl-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

edge claims as scientific and others as pseudo-scientific (or, as the Logical Positivists sometimes put it, for distinguishing sense from nonsense). The most well-known example of a demarcation criterion, still current in some scientific circles despite the fact that it has been fully exploded for more than half a century, is Popper's principle of falsifiability, according to which a theory counts as scientific if and only if it is potentially falsifiable. The history of attempts to devise a demarcation criterion is a history of failure too extensive to fully survey here. Suffice it to say that philosophers' persistent inability to formulate an adequate demarcation criterion has led most of them now to think that instead of searching for a single, universal test of epistemic adequacy, the best way to proceed is to do case-by-case studies of individual knowledge claims; Earman (2000, 3–4) concurs with this approach, as do I.

If Hume's maxim regarding miracles were a demarcation criterion, Earman would be right to be suspicious of it. But interpreting Hume's maxim as a demarcation criterion is a mistake. Earman supposes that Hume is offering an a priori argument against the possibility of the occurrence of miracles: Earman takes it that for Hume the probability that a miracle has occurred is "flatly zero" (Earman 2000, 13 and 23, *et passim*). In the framework of mathematical probability that Earman employs as the basis for his critique, $\Pr(p) = 0$ means that the probability of the proposition p is zero; that is, p is a logically false statement. Thus, *not-p* (the falsehood of p) is logically true or necessary. If p is a statement about the occurrence of a miracle, $\Pr(p) = 0$ means that the occurrence of the miracle is logically impossible. Earlier, I mentioned that it is a mistake to read Hume as asserting that the non-occurrence of miracles is logically necessary. Whether or not a miracle has occurred is a question regarding a matter of fact, and for Hume degree of belief for or against a matter of fact can never reach the level of perfect certainty or logical necessity (demonstration), only moral certainty (proof). (While Millican [2011] and [2013] agrees that Earman's account is flawed, he also argues in Bayesian terms that Hume's maxim is flawed. I address this indirectly in later chapters.)

An even stronger reason to think that Earman is mistaken on this point is that, as shown above, the whole structure of Hume's argument against miracles is a posteriori (and hence cannot lead to logically necessary propositions). Hume argues that *as a matter of fact*, and *given what we know* about human psychology and the facts of history—especially what we know about bogus miracle claims, the credulity of religious believers, and how humans come to know the laws of nature—there has never been and very probably never will be an instance in which the probability that a miracle has occurred rises to a level greater than the probability that the reporter is mistaken, has been deceived, or is a deceiver. It is true that Hume puts this point very strongly but, in the context of Hume's

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

thought, it is easy to see that despite his rhetoric he does not really mean anything stronger than this.

So, Hume's argument against miracles neither depends upon nor does it result in a demarcation criterion. Rather, Hume is making a general a posteriori judgment about the facts of experience that are relevant to specific judgments about purported singular occurrences. These facts are so well-established that we need not perform a detailed analysis in every case. This is consistent with common sense and good epistemic practice. Given this, a great many of Earman's criticisms are moot, since they are based on thinking that for Hume the probability that a miracle has actually occurred is always flatly zero.

Even though Hume is not proposing a demarcation criterion for deciding the incredibility of miracle reports, one might still complain that Hume does not go into enough detail to prove his claim that *every* known case of a miracle report has been a case of mistake or deception. It is true that Hume did less than he might have in this regard, but doing so certainly would have caused the chapter to balloon out of control, well out of the parameters of the *Enquiry*. Hume does discuss some of the then-recent and highly celebrated purported miracles worked at the tomb of the Abbé Paris, in order to show how that type of analysis would go (see EHU 10.27 and its long footnote). In essence, Hume argues that where religion and enthusiasm are involved, many witnesses are likely to be found, who cannot be contradicted except in virtue of the virtual impossibility of the thing to which they testify. As Hume says at the end of the discussion of another case immediately preceding that of the Abbé Paris: A just reasoner concluded in that case that "such an evidence carried falsehood on the very face of it, and that a miracle, supported by any human testimony, was more properly a subject of derision than of argument" (EHU 10.26). Note that Hume does not here pretend that he has a definitive disproof of these miracles; he is merely describing what he takes to be a reasonable attitude toward claims about the occurrence of things which are otherwise known to be practically impossible.

Beyond that, it is enough for Hume's purposes that reasonable people will be able to furnish particular evidence from their own experiences to make the general claim against miracles plausible for themselves. As Hume says, "I flatter myself, that I have discovered an argument . . . which, if just, will, *with the wise and learned*, be an everlasting check to all kinds of superstitious delusions" (EHU 10.2: emphasis added). In the same way that no reasonable person today puts enough stock in claims about UFOs to demand an extremely detailed assessment of the evidence in every individual case, reports of miracles can be safely dismissed without detailed individual analyses. The example mentioned in chapter 3 about the load of tile spilling to form an image of Elvis at the last supper illustrates the same general point. It is a moral certainty (given what we know) that these types of events do not occur. Even the wise and learned

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

could be mistaken in this sort of judgment—moral certainty is not certainty, after all. The burden of proof, however, is on those who wish to establish that a miracle (or even a marvel) has occurred. Given the nature and extent of the evidence in favor of the laws of nature, this burden is a heavy one indeed.

THE THRESHOLD FOR REASONABLE BELIEF

Earman also misses Hume's meaning when he gives a long discussion of results in Bayesian probability theory that collectively show that testimony or other evidence could potentially supply warrant adequate for rational belief in the occurrence of a miracle. The mistake here is that Earman relies in these calculations on preconditions or assumptions that Hume would allow hypothetically or ideally, but which he would utterly reject in the analysis of actual cases. For example, Earman's argument that the testimony of multiple witnesses could raise the probability of the occurrence of a miracle above the threshold for reasonable belief (Earman 2000, 53–9), depends on the assumption that all the witnesses are honest and have correctly perceived the event in question! This condition, Hume would say, might well be satisfied in exceptional cases. But given our background knowledge it is a practical certainty that we will never have adequate grounds to *believe* that this condition is in fact satisfied in any particular case. Whether the testimony is good in this sense is precisely what needs to be verified in order to be able to judge whether or not the testimony establishes the occurrence of a miracle; we cannot assume it in advance.

Earman's attack on Hume depends on taking the threshold for reasonable belief in some proposition p to be that there is a greater than 50 percent chance that p is true (Earman 2000, 41, *et passim*). This is the standard threshold degree of probability for reasonable belief in Bayesian analyses since, when the evidence makes the chance of a proposition slightly higher than fifty percent, the proposition becomes "more likely than not." Bayesians pick this threshold in part because it is difficult to come up with a principled justification for picking any other threshold degree of belief. Even the "95 percent confidence level" often used in statistics as the standard for a sufficiently well-established hypothesis is an arbitrary choice; the conventions for picking a confidence level are different in different fields, in part based on the field's intuitions about how important it is not to be wrong.

Now, since Hume speaks constantly in terms of probabilities and degrees of belief but never discusses the probability of belief in numerical terms, it is difficult to say what Hume would take as the numerical threshold for reasonable belief. As I argued in chapter 4, this is something that Hume would not be willing to specify at all, since he does not accept

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

the applicability of numbers to empirical probabilities. Of one thing I am quite sure, however, whether or not the account of Hume's theory of probability I outline is correct: That is, that if Hume *were* to attach numbers to degrees of belief, then the threshold for reasonable belief would depend on the knowledge claim in question. The more extraordinary the claim, the more extraordinary must be the evidence that would be required in order to establish belief in the claim as reasonable. In short, if Hume were to attach numbers to probabilities he would require a very high probability from the evidence to confidently assert that a law violation had actually occurred. And—crucially—even *ordinary* beliefs Hume would not think of as adequately warranted if there were a nearly fifty percent chance of them being false. This is precisely in the range where a good skeptic like Hume would suspend judgment and withhold belief altogether, since both the assertion and its denial are nearly equally likely. This is just one of the foundational differences between Hume and the Bayesians.

THE STRAIGHT RULE OF INDUCTION

Earman attributes the naïve "straight rule" of induction to Hume: "If [some number] n As have been examined, all of which are found to be Bs, then if n is sufficiently large, the probability that all As are Bs is 1" (Earman 2000, 23). He charges that Hume's "[S]traight rule of induction is both descriptively inadequate to actual scientific practice, and it is stultifying to scientific enquiry" (2000, 31). If true, this is a damning charge against Hume. However, the textual evidence Earman cites to support the claim that Hume advocates the straight rule of induction is slight at best—he quotes from four passages of the *Enquiry* (EHU 10.4, EHU 10.6, EHU 10.12, and EHU 10.12). None of them come close to asserting or implying the straight rule when Hume's epistemology is properly understood.

The text Earman cites that most strongly suggests the straight rule is this: "[I]t seems evident, that, when we transfer the past to the future, in order to determine the effect, which will result from any cause, we transfer all the different events, in the same proportion as they have appeared in the past" (EHU 6.4, quoted by Earman 2000, 81). But what Hume means by this is just that, through the habits of the mind that are involved in constructing causal laws, we form an expectation that the proportion of future effects resulting from present causes will resemble the proportion of such effects that followed similar causes in the past. The strength of this expectation (what Hume would call the degree of vivacity of the belief) depends on the character of our past experience. The more extensive our past experience, and the more regular it is, the more strongly we believe that the resemblance between future events and past events

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

will hold up. Even in the case of very extensive experience of a perfect regularity in past experience, however, Hume does not think the probability is 1 that the future will resemble the past. He admits, for example, that it is possible that the sun will not rise tomorrow (EHU 4.2), and that bread will not be nourishing the next time we eat it (EHU 4.16). For Hume, *no* amount of past experience could make it that "the probability that all As are Bs is 1." As I discuss in detail in chapter 7, Fogelin (2003, 43-53 and 58) gives an extended argument attempting to show why Earman is wrong to attribute the straight rule of induction to Hume.

Holding the mistaken view that Hume adopts the straight rule leads Earman to claim that

> Hume is saying that when experience is uniform—when sufficiently many As have been examined and all have been found to be Bs—then we have a "proof" that all As are Bs. In the section [6] of the *Enquiry* entitled "Of Probabilities," Hume divides arguments into demonstrations, proofs, and probabilities. Proofs are defined as "such arguments from experience that leave no room for doubt or opposition." In the probabilistic language I will adopt . . . this seems to imply that when experience provides a proof, the conditional probability of the conclusion, given the evidence, is 1. (Earman 2000, 23)

This gets Hume wrong in a fundamental sense, as I have already shown: Matters of fact can never be certain; moreover, since induction is fallible, Hume would never have intended to suggest that even a very large number of uniform observations produce a probability of 1. This is true even for the exceptionless regularities upon which laws are founded. When Hume says "no room for doubt" here, he means that we have a strong psychological tendency to expect that the contrary will not occur. There are *no grounds for doubt*, but this is not the same as saying that *the contrary is impossible* (the former is an epistemic claim, the latter a logical one).

Earman's misinterpretation of Hume on this point leads to other problems. One example is Earman's summary of the structure of Hume's argument regarding miracles: "So here in a nutshell is Hume's first argument against miracles. A (Hume) miracle is a violation of a presumptive law of nature. By Hume's straight rule of induction, experience confers a probability of 1 on a presumptive law. Hence, the probability of a miracle is flatly zero. Very simple. And very crude" (Earman 2000, 23). This is indeed simple and crude. But the simplistic and crude nature of this argument is good reason, I take it, for thinking that someone of Hume's acumen and sophistication would offer no such argument. In large measure, it seems to me, Earman's mischaracterizations of Hume are based on not understanding that "proof" for Hume is a probabilistic category. As I have already shown here, proof for Hume comes by degrees and has several kinds.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

In a letter to John Stewart, Hume gives a very explicit statement of his position on proof and kinds of certainty.

> But allow me to tell you, that I never asserted so absurd a Proposition as *that any thing might arise without a Cause*: I only maintain'd that our Certainty of the Falsehood of that Proposition proceeded neither from Intuition nor Demonstration; but from another Source. *That Caesar existed, that there is such an island as Sicily*; for these Propositions, I affirm, we have no demonstrative nor intuitive Proof. Would you infer that I deny their Truth, or even their Certainty? There are many different kinds of Certainty; and some of them as satisfactory to the Mind, tho perhaps not so regular, as the demonstrative kind. (Grieg I, 187)

Besides these differences between Hume's position on proof and kinds of certainty and Earman's interpretation of Hume on these issues, Hume and Earman also fundamentally disagree on the nature of probability itself, to the discussion of which I now turn.

HUME AND EARMAN ON PROBABILITY

Earman also cites the following passage as evidence that Hume adopts the straight rule: "Suppose. . . . I have found by long observation that of twenty ships which go to sea, only nineteen return. Suppose I see at present twenty ships that leave the port: I transfer my past experience to the future, and represent to myself nineteen ships as returning in safety, and one as perishing" (THN 1.3.12.11).

In Hume's example of the ships leaving and returning to harbor, the total number of observed ships is presumably very high—we have discovered the proportion "by long observation." Imagine that the number of ships is low, perhaps only twenty. What would Hume say then? Hume would say that we still generalize from this limited past experience and project the observed ratio of return onto future voyages, but our degree of confidence will be less strong than it would be if our past experience in this matter were more extensive. We expect the percentage of future returning ships to resemble the percentage that have returned in the past, but the degree of confidence is not high. To express this in a quasi-statistical form, we might say that we believe that x percent of the ships will return, and that we believe this with a y percent degree of confidence. If the regularity holds up as the extensiveness of our experience grows, our degree of confidence, y, will increase. For reasons discussed thoroughly earlier, we know that Hume would not have expressed his degree of confidence in this numerical fashion, but in his account a similar kind of work is being done by degree of vivacity of belief. Notice that the fact that Hume's degree of belief varies with the extensiveness of the evidence—even if the specific regularity in question (proportion of returning ships)

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

stays the same—is another reason why it is clear that Hume is not employing the straight rule.

Here is an application of the straight rule of induction, one that is deliberately designed to show how silly that inference rule is. The number of observed universes, N_o, is 1. The number of observed universes that harbor life, N_l, is 1. "The probability that any universe has life, based on observed data, is given by $N_l / N_o = 1$ (100%). The statistical error is, needless to say, large" (Shanks 2004, 215). The "statistical error" Shanks is talking about represents the chance that the ratio mentioned yields the correct value for the probability that a universe contains life. Due to the small sample size, the margin of error in the sample inference is large. Although Hume does not discuss statistical notions such as margins of error and confidence levels (since they weren't yet invented!), it is interesting to note that in a significant sense his theory of evidential reasoning builds in resources that do much the same work. When our sample size is small, statistics tells us that the margin of error is large. This is to say that we should not be very confident that the true value of the parameter in question is the measured value—the true value could be significantly above or below the measured value. The larger the sample, the smaller the margin of error. For Hume it is both the regularity itself *and* the extensiveness of our past experience of it that gives us our degree of belief in empirical generalizations.

Earman writes,

> [I]f the weighing of proof against proof is to be done within the ambit of the probability calculus and the rule of conditionalization, then Hume's straight rule has to be dropped—his proof in favor of [a law statement] L by uniform experience cannot be taken to mean probability 1 but at most a high probability that is short of 1. Consequently, uniform experience does *not* furnish a proof against a miracle in the sense of making the conditional probability of its occurrence flatly zero, although this probability may be very, very tiny. (Earman 2000, 32)

Earman takes himself here to be revealing an inconsistency in Hume's position. Earman thinks Hume does (and should) work within the probability calculus, and that he adopts the straight rule. The first is incompatible with the second, so Earman thinks it is a victory to claim that Hume must drop the straight rule and that at the same time this undercuts (what Earman represents as) Hume's position that the probability of the occurrence of a miracle is flatly zero.

By now my response to this will be quite predictable: For Hume, the weighing of proof against proof is not to be done within the ambit of the probability calculus and the rule of conditionalization; Hume does not have to drop the straight rule, because he never held it; "proof" for Hume never meant a probability of 1 anyway; and Hume never claimed that

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

experience makes the probability of the occurrence of a miracle flatly zero.

I will conclude this section by commenting on one final passage from Earman:

> A number of Hume's contemporaries, such as Price, understood Hume's claims as being about quantifiable degrees of belief or credibility, the quantification being subject to the constraints of the probability calculus. I have no doubt that Hume would have agreed to this much, and I have little doubt that…he would have agreed that the probabilistic form of analysis is wholly appropriate when discussing the credibility of testimony. Naysayers will have a hard time explaining away [the letter from Hume to Price (Klibanski and Mossner 1954, 233–34)], where Hume is implicitly accepting the probabilistic form into which Price cast Hume's argument. (Earman 2000, 25)

In chapter 4, following Coleman and Cohen and relying on Franklin's history of evidential probability, I showed in detail that the propositions Earman has no doubt Hume would accept are ones Hume would in fact deny. As a "naysayer" I have an easy explanation of the letter to Price. Rather than "implicitly accepting" Price's mathematical re-casting of his argument, Hume's remarks are really quite non-committal. All he says is that Price's argument is "new and plausible and ingenious, and perhaps solid" and that he needs more time to judge it. Hume was always cordial to critics who treated him cordially (see Coleman 2001, 196–197). Politeness is thus a sufficient explanation of Hume's letter to Price. It would be perfectly consistent for Hume on reflection to have maintained his position (previously implicit) that numbers are inappropriate in the assessment of evidential probabilities.

IS THE NON-MATHEMATICAL APPROACH TO EVIDENTIAL PROBABILITY PLAUSIBLE?

The previous chapter argued that Hume's non-mathematical approach to evidential probability is consistent with a long and laudable tradition. But history aside, Hume's approach to evidential probability might even be correct. Certainly, the non-mathematical approach is still commonly agreed to be appropriate in many kinds of situations. As Franklin (2001, 131) emphasizes, it is very unusual in law, science, and ordinary life to treat probability numerically: "The big bang theory of the universe is much more probable, on present evidence, than the steady-state theory. But it is a rare scientist who can be found to say exactly how much more probable—or even approximately how much." Part of Franklin's thesis is that the historical scarcity of numerical treatments of probability is not a sign of the underdevelopment of probability theory before Pascal, nor even a sign of the difficulty of applying numerical probability in practical

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

situations. Rather, it is due to the fact that in many situations the numerical approach to probability is simply inappropriate.

The mere existence of the long and noble tradition of non-mathematical evidential probability is, of course, not enough by itself to show that Hume and others were *correct* to reason about evidential probability in the non-mathematical way. The justification question comes down to the issue of whether or not all types of probability can sensibly be treated numerically. I do not have a definitive argument that there are kinds of probability that *must not* be analyzed numerically. But various considerations, besides its long history, make the non-numerical approach plausible and worthy of further consideration. As Franklin puts it,

> Factual probability is essentially numerical, certainly. And the standard mathematical theory of probability treats only of probabilities which are numerical. But Keynes, in his classic *Treatise of Probability* [1921], argued at length that not all logical probabilities should have numbers. Even if they do have numbers in principle, no convincing way has been discovered of actually assigning a number to, for example, the probability of the steady-state theory of the universe on present evidence—or even such a simple case as the probability that the next ball chosen from an urn will be black, given that all of the twenty balls already chosen have been black. . . . This should caution us against supposing that, because the concept of probability before Pascal was mostly non-numerical, it was therefore primitive or in some way inadequate. (Franklin 2001, 327)

Even in our own time, as Franklin (2001, 22) points out, the central problem of determining the probability of propositions supported by exceptionless past experience has resisted all attempts at mathematical analysis. Moreover, the debate between propensity and frequentist interpretations of probability has not been settled.

> The debates of the time of Laplace on the principle of insufficient reason never satisfactorily resolved whether the probability of a coin's landing heads is a half because the coin is symmetrical, and hence there is no reason to prefer heads to tails, or because many throws of coins have been observed to produce about half heads and half tails. Keynes' chapter on the weight of evidence shows that we are still no closer to explaining the difference between the probability of a hypothesis in the two cases in which there is little evidence with a certain probability on balance and in which there is a great deal of evidence with the same probability on balance. Are there two dimensions of probability, one giving the total probability and the other the firmness or weight with which that probability can be held? (Franklin 2001, 78)

All this lends plausibility to the idea that mathematical probability is not the only viable approach to problems regarding weight of evidence. It is worth remarking that adopting a non-numerical theory of evidential probability does not mean that we must give up all hope of precision and

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

rigor in our assessments of the probability of empirical hypotheses. There are other ways to achieve this: In discussions of temperature, for example, classificatory concepts (hot, warm, very cold, etc.) and comparative concepts (hotter than, colder than, etc.) are possible without any numerical scale. Similarly, the non-numerical tradition of evidential probability has given us perfectly serviceable classificatory concepts (improbable, probable, highly probable) and comparative concepts (more probable than, less probable than, etc.). (See Franklin 2001, 328.)

If this seems insufficient, we should ask ourselves whether quantitative theories of probability can really do any better: Has the quantification of probability helped in the evaluation of uncertain evidence in science? Franklin's answer is that, "In the restricted cases in which statistical tests apply, it has, but for more general theory evaluation, it seems not" (Franklin 2001, 369). It seems clear to me that Franklin is correct about this.

As further support for the plausibility of the idea that there is a sensible non-numerical concept of evidential probability, Franklin points out that for each of Huygens, Newton, and Darwin, the evaluation of evidence is an entirely non-mathematical affair. The names on that list could be multiplied *ad nauseam*.

Franklin also reminds us that even today there are many areas in which we still rely on non-numerical concepts of evidence and probability. In risk assessment, for example, we use expert panels, not math. In attempts to implement probable reasoning in artificial intelligence systems, even the most basic facts about ordinary probabilistic reasoning have resisted all attempts at formalization. And in modern law, "it is accepted . . . that such notions as proof beyond reasonable doubt involve probability. But all attempts to quantify the concept have been resisted" (Franklin 2001, 365).

Although "proof beyond reasonable doubt" appeared in English law around 1770, it has never been treated numerically in the courts. Franklin (2001, 366) cites deliberately non-numerical legal explanations of that concept from as recently as 1981.

Above, I mentioned that the English courts have not allowed any attempt to have jurors use formal mathematical models, such as Bayesianism, when evaluating evidence (Roberts and Aitken 2014, 104-106). This is not to say that expert witnesses must not use Bayesianism in formulating their conclusions, for example about DNA evidence; forensic experts use Bayesianism in their work, and this is part of the source of their expertise. It is just that *jurors* are not to use (or be expected to use) such mathematical tools, since they are beyond what common law expects the "reasonable person" to know.

It is interesting to contrast statistical DNA evidence and the purported evidence for miracles here. DNA, made up of combinations and patterns of four distinctive chemicals, can be analyzed both via "Monte Carlo"

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

models of random variation and via comparing samples of DNA across huge numbers of individuals. Thus, a judgment that the DNA found at the crime scene has some statistical chance of being the DNA of a specific subject (colloquially, a "DNA match") is based essentially on a frequency in a large enough sample that the probability of error is low (and the confidence level is high). The evidence for a miracle is nothing like this sort of robust statistical evidence. Instead, in the case of a miracle we have one or at best a very small number of witnesses reporting the purported miracle, not to mention that we have independent reasons to doubt their reports. Whereas in large statistical samples (or over the long run), we expect any errors in the assigning of subjective *prior* probabilities to be "washed out" in the sense that whatever prior probabilities are assigned by different epistemic agents at first, in the long run Bayesian updating causes all of the agents's subjective *posterior* probabilities to converge on the same value. In a tiny sample such as that available in the case of a miracle report, we have no such luxury. The subjective prior probabilities assigned by different agents will completely dominate their judgment about the occurrence of the miracle. In the long run, Bayesian updating on evidence from large samples gives solid results; after just a single trial, Bayesian results are more or less nonsense, entirely dominated by subjective (and likely irrational) prior probabilities.

A better, more Humean, way of applying Bayesianism in this context would be to use Bayes's Theorem to assess the probability of a universal empirical generalization given a long, extensive, and perfectly regular experience across the whole of humanity. After these billions or trillions of trials, the posterior probability of the generalization would be extremely high, very nearly 100 percent. Then, add in a single, dubious claim regarding the occurrence of an exception to that empirical generalization: The Bayesian updating would not perceptibly change the posterior probability of the generalization; that is, the miracle report would provide no rational grounds for believing that the law of nature had been violated. Note that I called this a "more Humean" approach. It is still not a *Humean* approach since Hume himself would not attach mathematical probabilities to evidence or to empirical conclusions at all.

It seems that Aristotle had it right: "It is the mark of an educated man to look for precision in each class of things just so far as the nature of the subject admits" (*Nicomachian Ethics*, 1094b24–25). The power and precision of mathematics is wonderful. It has had practical and epistemic benefits in many areas where it has been applied, including many areas dealing with probability. Nevertheless, it would be a mistake to therefore expect that *all* instances of probability can be quantified in a meaningful way.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

WHAT ABOUT THE DUTCH BOOK ARGUMENT?

Bayesians often appeal to the Dutch Book argument as if it establishes definitively that the mathematical approach to probability is the only correct approach. The Dutch Book argument aims to show that someone whose reasoning about probabilities is not in conformity with the axioms of mathematical probability is susceptible to willingly accepting a "Dutch Book." A Dutch Book is a finite series of bets each of which the bettor takes to be fair but over the course of which the bettor is guaranteed to lose money, no matter what the chance outcomes of each bet. People whose judgments about the fairness of bets conform to the axioms of mathematical probability theory will not fall into this trap. Since Bayes's Rule, which is designed to tell you how to update your degree of belief in light of new evidence, is a simple consequence of the axioms of mathematical probability, the Dutch Book argument is taken to imply that it is necessary to be a Bayesian about evidential probability. (See for example Talbot [2011] on this.)

Bayesians often put the point by saying that Bayes's Rule is a *condition for rationality*: If you reason about probability in some way that is not in accordance with the axioms of mathematical probability, you fall into probabilistic incoherence. Earman (2000, 26) gives a précis of the Dutch Book argument, and later appeals to it as providing a persuasive argument that the axioms of probability are conditions of rationality: "Any procedure that proportions degrees of belief in violation of the axioms of probability or that dictates belief change that is in violation of the rule of conditionalization [i.e., Bayes Rule] is irrational—or at least there are arguments of some persuasiveness in favor of such a position" (Earman 2000, 29–30).

A Bayesian might hope to find here a demonstration that the non-Pascalian approach to probability is doomed to failure. Note, however, that the Dutch Book argument *assumes* the Pascalian scale of probability, it does not prove it. Without the assumption that degrees of belief are numerical and run on a continuous scale from 0 to 1, the basic arithmetic of the Dutch Book argument would be impossible. So, what the Dutch Book argument actually shows is that *if* there are numerical degrees of belief ranging continuously from 0 to 1 that apply in a given context, *then* one must, on pain of incoherence, reason about degree of belief in conformity with the axioms of mathematical probability (and hence with Bayes's Rule). A hypothetical proposition of that sort, even if true, does not prove that one *must* be a Pascalian in the first place.

Howson and Urbach argue that "standard" invocations of the Dutch Book argument falter over the fact that "the postulate, that degrees of belief entail a willingness to bet at the odds based on them, is vulnerable to some telling objections" (2003, 90). Among these, for example, is the fact that it may be smart in some situations to bet against your actual

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

degrees of belief—when, say, you are considering a universal generalization which can only be falsified and never verified, it is wise to bet that its probability is zero, even if you believe it to a non-zero degree. Howson and Urbach develop a "non-standard" version of the Dutch Book argument that avoids this and other potential problems, but it is admittedly based on a purely fictional situation in which the propositions over which there are wagers have definite truth-values that are revealed to the bettors and all debts are collected.

I find the Dutch Book argument convincing for cases involving chance setups where the numerical frequency of possible outcomes is known in advance. However, for other sorts of cases, such as ordinary scientific reasoning or in the law, I do not see that it has compelling force. Since the space of alternatives is not known (and if the underdetermination thesis is correct, it cannot be known), we cannot say which distribution of degrees of belief is maximally rational according to the probability calculus.

Here's another consideration: If you shoehorn mathematical probabilities onto epistemic probabilities they don't fit, aren't you then likely to commit a Dutch Book inadvertently? Isn't that a good reason to avoid the illusion of mathematical precision when we don't know the mathematical probabilities, or when they don't apply?

POTENTIAL ANTI-BAYESIAN ARGUMENTS

So far, I have argued that there are no convincing positive reasons in favor of the claim that Bayesianism always applies to all cases of probable reasoning. Are there, in addition, negative reasons that would lead one to conclude that Bayesianism is a flawed approach that should be rejected in at least some kinds of cases? Is anything *wrong* with Bayesian analyses of evidential probability?

Bruce Glymour thinks that, "relatively few Bayesians are actually persuaded of Bayesian doctrine by Dutch Book arguments, stable estimation theorems, or other a priori arguments. Their frailty is too palpable" (Glymour 1980, 74). Instead, he sees two considerations as drawing people to Bayesianism as a theory of confirmation: First, Bayesianism makes weak assumptions about prior probabilities and yet often seems to come up with results that accord with our intuitions and practices and, second, it is so flexible and apparently precise that Bayesians hope to be able to explain "the vagaries of scientific reasoning" (Glymour 1980, 74). His view, in contrast, is that, "particular *inferences* can almost always be brought into accord with the Bayesian scheme by assigning degrees of belief more or less ad hoc, but we learn nothing from this agreement. What we want is an explanation of scientific argument; what the Bayesians give us is a theory of learning, indeed a theory of personal learning" (Glymour 1980, 74). He goes on to raise a series of specific critiques of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

particular aspects and applications of Bayesianism (some of which have been answered in subsequent literature). His conclusion is that while none of his arguments are decisive against Bayesianism, "taken together, I think they do at least strongly suggest that there must be relations between evidence and hypothesis that are important to scientific argument and to confirmation but which the Bayesian scheme has not yet penetrated" (Glymour 1980, 93).

One problem with Bayesianism can be teased out this way. Bayes and Price (and, by extension, other Bayesians such as Earman) are forced to make assumptions about the distribution of chances across contrary possible outcomes, assumptions which are rarely if ever justified outside of highly constrained and artificial experimental situations. Bayes himself, for example, develops his argument in terms of the equal chances of a perfectly round ball coming to rest at any given place on a perfectly flat table. His conclusions do indeed follow for such idealized cases. But, to speak metaphorically, we usually do not have round balls and flat tables—or at least we cannot be sure that we do. The assumption of the equipossibility of contrary outcomes is therefore usually not justified in actual cases. Barry Gower writes, "Hume had, in effect, noticed the role of an assumption of this kind in any attempt to counter inductive skepticism when he pointed out that 'if there be any suspicion that the course of nature may change, and that the past may be no rule for the future, all experience becomes useless and can give rise to no inference or conclusion'" (Gower 1997, 103, quoting EHU 4.21).

It may be that, methodologically speaking, when assessing probabilities numerically there is no *better* principle to apply than the principle of indifference (which says that contrary possibilities are to be treated as equally probable in the absence of any reason to think otherwise), but that does not mean that it is a *good* principle to apply in assessing the evidential probability of empirical hypotheses in the sense that it reliably leads to correct conclusions in all types of cases.

A related point has been made in the context of using Bayesian methods in the analysis of forensic evidence in the law:

> Bayes' Theorem does not supply (and as a theorem, should not be expected to supply) prior probabilities from which to construct prior odds. Real-world forensic applications of Bayes' Theorem, in other words, necessarily rest on subjective human judgements of "prior" probability. Consequently, any resulting inferences of probative value extracted from Bayes nets can only be as good, or bad, as the initial human inputs. It is salutary to remember this at all times, lest the allure of quantified posterior probabilities should produce any "grand illusion" (Callen 1982) of finality, exhaustiveness or non-contestability. (Roberts and Aitken 2014, 104)

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Bayesians normally argue that although initial prior probabilities are subjective and thus may be biased or otherwise badly flawed, in the long run when many new pieces of evidence are brought to bear those initial prior probabilities are "washed out" and everyone's posterior probability converges on the same value regardless of where they started with their priors. One problem with applying Bayesian updating to legal or even scientific contexts (for example, in cosmology) is that there are not enough runs to wash out the priors, and thus the resulting posterior probabilities are mostly due to the subjective priors used as inputs.

Susan Haack supplies related grounds for eschewing Bayesian probability when analyzing legal evidence, or any evidence where degrees of warrant (rational credibility) are in question. Her critique of subjectivist Bayesianism in the law is therefore generalizable to the kinds of cases Hume is concerned with in the evaluation of the probabilities of empirical matters of fact. She writes, "The mathematical calculus of probabilities is perfectly fine *in its place*; but that place is a limited one. In particular, this mathematical calculus sheds little or no light on the crucial concept [Bertrand] Russell [1948, 381] calls 'rational credibility' and I call 'warrant.' . . . [I]t follows from my epistemological analysis that degrees of warrant cannot be identified with mathematical probabilities" (Haack 2014, 47). Haack adduces several reasons for thinking that, "the mathematical theory of probabilities couldn't possibly, by itself, constitute a theory of warrant" (61). I will mention two of her reasons as especially probative in the case of Hume and miracles. First, "The mathematical probability of (p and not-p) must add up to 1; but when there is no evidence, or only very weak evidence, either way, neither p nor not-p is warranted to any degree" (62). Hume would certainly agree with this in the case of contrary matters of fact for which we have insufficient evidence either way. Second, "The mathematical probability of (p & q) is the product of the probability of p and the probability of q—which, unless both have a probability of 1, is always less than either; but a combined evidence may warrant a claim to a higher degree than any of its components alone would do" (62). Hume would find this persuasive, too, I think.

We might still find grounds to criticize Hume or other historical figures for not using the mathematical theory of probability, but such criticism should not beg the question by merely assuming the universal correctness of the mathematical approach. And even if we decide that the non-numerical approach to probability is doomed to failure, we should avoid misinterpreting historical figures by imposing the framework of mathematical probability upon their ideas. That would be the height of revisionism; we would then be attacking straw figures, not the real ideas of actual historical figures. The principle of the charity of interpretation advocated in good historical scholarship at a minimum requires that we

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

interpret historical figures on their own terms first, even if we later go on to show that their fundamental assumptions are mistaken.

Earman (2000, 25) chastises Hume for being unaware of Bayesianism and of mathematical probability generally (Bayes 1763). This is unfair on two counts. First, Bayes's work on probability was not widely known in 1748 when Hume published the first edition of the *Enquiry*. Richard Price arranged the posthumous publication of Bayes's essay only in 1763, and it remained obscure even after its publication. Price's own paper applying Bayesian methods to the evidence for miracles appeared in 1767. We know Hume read and apparently admired that paper (Klibanski and Mossner 1954, 233–34; quoted in Earman 2000, 24), but still Hume neither addressed Bayesian arguments nor revised his account of miracles for the 1768 or 1777 editions of the *Enquiry*. This suggests that Hume ultimately did not view Bayes's work as relevant to the argument against miracles.

The second sense in which Earman's charge that Hume is unaware of mathematical probability is unfair is that Hume's discussion of the probability of chances (in, for example, THN 1.3.11 and 1.3.12, and EHU 6), "shows without controversy that he was familiar with the basic concepts of probability based on the calculus of chances" (Coleman 2001, 201). As a student, we know that Hume studied some mathematics; he even transcribed a textbook on fluxions (i.e., differential calculus; Harris 2015, 41). Given Hume's familiarity with Pascalian probability, and his acquaintance through Price with Bayesian ideas, his non-numerical treatment of the evidential probability of miracles even in the latest editions of the *Enquiry concerning Human Understanding* must be seen as a deliberate philosophical position, not as a result of negligence or ignorance.

Jaynes (2003) uses Cox's consistency theorems to justify a logical interpretation of probability that leads to a Bayesian framework for analyzing evidential relations. The first of the three assumptions from which Cox derives the axioms of probability is that "(1) Degrees of probability are to be represented by real numbers" (Jaynes 2003, 656). Jaynes appeals to a pragmatic reason for accepting this assumption (namely, it is impossible to see how to program a machine to calculate probabilities unless they are associated with some definite physical quantity) and then provides a seemingly more rigorous justification based on breaking down assumption (1) into two components, namely transitivity and universal comparability. Jaynes suggests that most any conception will want probability to satisfy transitivity and universal comparability. Since together they entail an analysis equivalent to the real numbers he asserts, "it would seem foolish not to use the great convenience of the numerical representation" (Jaynes 2003, 657). But this is not a proof (in anyone's sense) that every theory of probability *must* embody transitivity and universal comparability.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

A PARTIAL CATALOG OF BAYESIAN HUMES

Earman (2002, 92) doubles down on the claims of his (2000) book: "I claim that when Hume's 'Of Miracles' is examined through the lens of Bayesianism, it is seen to be a shambles."

Earman was of course not the first to give a Bayesian analysis of Hume's argument against miracles. There have been a great many such attempts over the years, beginning with Richard Price (1767). Interestingly, about as many of these Bayesian accounts aim to support Hume's conclusion as aim to refute it. The fact that contradictory results have been achieved is, in my opinion, a symptom of the plain wrong-headedness of Bayesian reconstructions of Hume's argument against miracles. It is like trying to use a hammer to install a light bulb: You end up with bad results, but that does not mean that there is a fundamental flaw with the light bulb.

Mandelbaum (2018, 1) mentions that, "Bayesianism, in one form or another, has never been more popular than it is now," and he goes on to give a long (but still not complete) list of diverse areas of philosophy and cognitive science in which Bayesian approaches have recently figured prominently. We could add many areas of the physical, biological, and social sciences, as well. Even the website RateBeer.com uses a Bayesian algorithm to compare user ratings across beers. Mandelbaum cuttingly adds, "The sheer generality of Bayesianism allows a scope unmatched by most theories, save for discredited ones like Radical Behaviorism and Associationism" (2). This comparison may be slightly unfair—after all, Bayesianism is supposed to be just a mathematical framework and mathematical frameworks are often thought to be content neutral and hence widely applicable. Saying that the sheer generality of arithmetic gives it a scope unmatched by few but discredited theories would not really make us doubt arithmetic. Yet I have to agree that the ideological fervor and rigidity sometimes displayed by Bayesians does remind me of the Behaviorists. In what follows, I list just a few Bayesian analyses of Hume to give the flavor of the variety of interpretations that have been offered.

Unlike Price and Earman, David Owen (1987) argues that a Bayesian analysis shows that Hume's position against the credibility of miracle reports is correct. Fogelin (2003, 47) thinks that Hume's position, "though mathematically naïve, is broadly Bayesian in character." Fogelin goes so far as to say that Hume is a better proto-Bayesian than Price (Fogelin 2003, 47)!

Jordan Sobel (1987 and 1991) is one who misreads what Hume means by "proof," claiming that Hume thinks the probability of a miracle occurring is zero. Making the probability of miracles zero is equivalent to saying that miracles are logically impossible. This has the undesirable consequence that Bayes's rule is then unable to increase the degree of belief in the occurrence of a given miracle *no matter what the evidence*.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Sobel "corrects" Hume here by treating the probability of a miracle occurring as infinitesimally close to zero. Other considerations aside, this seems a very un-Humean move.

Peter Millican (1993) proposes a Bayesian alternative to Sobel's account and criticizes several of its details and implications. Phillip Dawid and Donald Gillies (1989) offer a Bayesian analysis of Hume's argument which they think is simpler than Owen's and Sobel's analyses, and which they think makes clearer the differences between Hume and Price.

Millican (2003) provides a catalog of five (!) different Bayesian equations from the literature, each designed to capture what Hume meant by "no testimony is sufficient to establish a miracle, unless the testimony be of such a kind, that its falsehood would be more miraculous, than the fact, which it endeavours to establish" (EHU 10.13). Millican then argues that all five of the Bayesian equations he mentions are flawed because they misrepresent Hume, are implausible in themselves, or fail to establish the necessary and sufficient conditions Millican thinks Hume is after (Millican 2003, 5–6). Millican concludes that his own (1993) attempt and Earman's formulation that is equivalent are both unsatisfactory simply because that formulation is trivial in the way Earman says. For the more recent Millican, this means "we have yet to find a reading of Hume's maxim which is logically, textually, and epistemologically plausible" (Millican 2003, 10). I admire Millican's insistence on these principles of adequacy for judging interpretations of Hume. The new formula Millican then proposes as a simple, accurate, and plausible account in terms of *types* rather than *tokens* of probable evidence, however, still commits a deep error that is surprising in a Hume scholar of Millican's expertise. Namely, his formula commits Hume to the position that there are sufficient grounds for belief in the occurrence of a miracle when the opposing evidence from testimony and experience makes the probability of the miracle ever so slightly greater than fifty percent. As I remarked above, this is just the range where a good skeptic like Hume would be suspending belief, not accepting a radical claim, even if he did think that mathematical probability applied to the case. This, if I am right, is the very source of the difficulty Millican has in finding an appropriate Bayesian interpretation of Hume

Dorothy Coleman (1988) mentions several other works that address Hume's account of probability in terms of the mathematical theory of chances. In a similar vein, Sally Ferguson (2002) argues that "the attempt to apply Bayesian reasoning to the argument as presented in the [*Dialogues concerning Natural Religion*] is not well supported as a reconstruction of Hume's own approach" (113); "there are good reasons for not treating Hume's reasoning [in the *Dialogues*] as even proto-Bayesian" (114). Ferguson shows that "the benefits that have been claimed for [the Bayesian] approach, in terms of exposing both the subtleties of the argument and Hume's reasoning about it, can equally well be derived from a

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

careful analysis of the argument under a model of analogical reasoning, without the need of Bayes's theorem" (Ferguson 2002, 114). Ferguson also gives an excellent survey of attempts to interpret Hume through Bayes (114-18).

A detailed response to each of these Bayesian analyses is beyond the scope of this book—or more honestly, beyond my patience. Besides the specific rebuttals of Earman and other Bayesians offered here, it will have to suffice to note that if Hume is a non-Pascalian, as I have argued he is, then favorable Bayesian reconstructions of "Of Miracles" and other parts of Hume's works are just as misguided as the unfavorable ones.

The fact that there are so many incompatible Bayesian accounts of Humean probability and of his theory of miracles also suggests that the "correct" Bayesian account of the evidence for miracles is even now neither clear nor settled. This is yet another reason not to blame Hume for not providing a Bayesian analysis. Given the number and diversity of Bayesian interpretations, it should perhaps be no surprise that some of their conclusions are consistent with what Hume says about miracles—although in the present context I hesitate to calculate the numerical probability of such a coincidence.

> Bayesian analyses are prominent among recent and allegedly novel interpretations of Hume's argument. However, since there is no consensus on just what Hume's argument is, or exactly what he is trying to establish, it is impossible that any Bayesian analysis, or a recasting of the argument in terms of some version of Bayes's theorem, will not beg crucial issues of interpretation. In so doing, such analyses—in and of themselves—will also beg fundamental epistemological issues concerning, for example, evidence. Furthermore, it is difficult to see how recasting Hume's argument in a Bayesian form can clarify the structure or substance of the argument, as Earman claims, without presupposing what the argument is. (Levine 2002, 166)

I am sympathetic to the thrust of Levine's argument here, but I would add that even if there were broad consensus on the interpretation of Hume on miracles, recasting that argument in Bayesian terms would still misrepresent Hume's intent.

NOT BERNOULLI, EITHER

Barry Gower thinks that the

> "Bayesian" interpretation of the argument against miracles misrepresents Hume's reasoning. . . . [T]he error needs to be rectified, not just because it involves a mistaken view about the past but, more importantly, because it obscures the legacy of a different mode of thinking evident in Hume's writing about probabilistic inference which deserves to be recovered. Our thinking is impoverished if certain pre-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> sumptions about probability become so entrenched that we have great difficulty in seeing them as anything other than obvious. (Gower 1990, 17)

I could not agree more, except that the "different mode of thinking" Gower has in mind is a mathematical (but non-Bayesian) approach to evidential probability found in Jakob Bernoulli's (1713) *Ars Conjectandi* (the art of conjecture, or reasoning from partial evidence). Gower suggests that Hume's argument against miracles is an application of Bernoulli's approach. I see two difficulties with the Bernoulli hypothesis. First, a Bayesian could respond that belief coherence demands that mathematical probabilities conform to the probability calculus: Then, if Bernoulli-style reasoning leads to a conclusion different from that produced by Bayes's Rule, it can be rejected. That would imply either that Hume's argument must be equivalent to the Bayesian assessment of the probability arising from the testimony for miracles or that it must be wrong—a conclusion we should not be too quick to embrace. The second difficulty is that, like the Bayesian analysis, the Bernoullian analysis already assumes that evidential probabilities are numerical. If we deny this, as I think Hume would, then we also avoid the first difficulty. Rather than looking to Bernoulli in order to understand Hume's theory of evidential probability, we should look to the medieval legal tradition—a tradition whose mutual influence on Pascal, Bernoulli, and other developers of mathematical probability explains the similarity between their approaches and Hume's.

In most other respects Gower's position is parallel to the one advocated here. He thinks, for example, that "Hume's probabilities are not structured in accordance with any conventional theory of chances where they are represented by fractional numbers between zero and one" (Gower 1990, 22). He notes, too, that Hume's probabilities are non-additive, and remarks that "Hume's training as a lawyer may well have influenced his attitude to probabilistic reasoning, and it is recognized that legal probabilities" do not conform to the axioms of the probability calculus (Gower 1990, 21). This fits, of course, with Franklin's story about the history of probability, and with my argument that Hume is definitely and deliberately part of the ancient non-mathematical tradition of probable reasoning.

Gower gets things exactly right when he writes that Hume,

> quite evidently evaluates probable arguments in a way that cannot be reconciled with the pre-suppositions of Bayes's theorem. For example, an argument [with no] probability is, for Hume, one where favourable cases equal the unfavourable cases; and an argument where there were only favourable, or unfavourable, cases, would not be a probable argument at all [but instead would be a proof in Hume's sense]. We think that an event with a small probability is unlikely to occur; Hume

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> thought that such an event is more likely to occur than not. (Gower 1990, 24)

I took the liberty of replacing Gower's "of zero" with "[with no]" in the quotation above because it is very clear in the context that Gower does not intend to attribute a *numerical* conception of evidential probability to Hume. To my knowledge, Hume himself never uses the word "zero" when speaking of epistemic probability, though he sometimes speaks of claims regarding matters of fact as having "no probability."

PLURALISM ABOUT PROBABLE REASONING

In his elegant book, *Philosophical Theories of Probability* (2000), Donald Gillies advocates pluralism with regard to interpretations of mathematical probability. Gillies argues that there are some problem kinds (epistemic probabilities) that are best treated from the point of view of subjectivism (Gillies's examples are economics and other social sciences). Other problem kinds (objective probabilities) are best treated from the point of view of frequentism (Gillies's example is physical science). There is no single, universal interpretation because mathematical probability means different things in different contexts. Taking my cue from Gillies, I suggest an even more thorough-going pluralism. Probability in general means different things in different contexts, *and in some contexts, probability cannot be captured in a mathematical framework*. Hume, at least, is a pluralist about probability in this way. Like Gillies, Hume sees objective and epistemic probabilities as different in kind. He treats objective probabilities such as those having to do with dice in Pascalian terms, and he treats epistemic probabilities in non-Pascalian terms.

Franklin notes that "The fundamental problem in trying to apply mathematical probability to evidence in law (or, for that matter, in science) is that there seems to be no set of equiprobable basic alternatives" (Franklin 2001, 365). This is to say that we cannot estimate or calculate that the chance of something occurring is x out of y because we have no idea what the number of possible alternatives, y, is. Similarly, Barry Gower writes that, "Perhaps, indeed, the very idea of measuring degree of certainty is misguided, and the attempt to boost confidence in probable reasoning by developing a quantitative logic of reasoning is misconceived. For certainty is, in part, a psychological concept; people with different psychological characters may attribute different degrees of certainty to the conclusion of a probable argument" (Gower 1997, 91).

Gower goes on to make the point that, in that case, quantitative and qualitative judgments of one person's degree of certainty, as compared to another's, provide psychological information but not information about what the objectively best degree of certainty is. So, there is a normative problem even if we do get to numbers: "What is needed is the degree of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

certainty which a reasonable person would have in the conclusion of a probable argument" (Gower 1997, 91).

This is a key issue in evidential probability, and its discussion has a long history. For example, in the 1760s, Daniel Bernoulli and Jean d'Alembert debated whether or not a reasonable person should use the mathematical theory of probability to decide questions such as whether or not to be inoculated against small pox.

Bernoulli argued, on the basis of a probabilistic analysis of public health statistics available to him, that the increased life expectancy from small pox inoculation was four years, and that this increase in expected value was itself what made it reasonable to choose inoculation.

> D'Alembert accepted that reasonable people should choose inoculation. But he disputed the cogency of Bernoulli's argument. Reasonable people are not just calculators; they have psychological dispositions which have a bearing on the conclusions they reach. It may not be possible to give a quantitative form to these dispositions but we cannot and should not ignore them. For example, a reasonable person exercising reasonable caution would not exchange the certain prospect of one pound for a one in ten thousand chance of ten thousand pounds, even though the expected values of these prospects are, according to Bernoulli's methods of calculation, identical. (Gower 1997, 92)

D'Alembert argues that subjective *values* affect probability judgments. Such values are probably not quantifiable. D'Alembert thus challenges the very cogency of attempts to quantify evidential probability in science. To quote Gower again: "In identifying modes of reasoning which are legitimate in science we are, in effect, identifying modes of reasoning which a reasonable and rational investigator would use, and d'Alembert's view was that mathematical calculations do not always reflect the reasoning of such an investigator" (Gower 1997, 94). This line of analysis in effect entails that we should not expect a Bayesian analysis of evidential reasoning—or for that matter any mathematical analysis evidential reasoning—to yield good results. Mathematical probability applies perfectly in stochastic setups with known alternatives; in other contexts, it is an artificial tool that does not accurately reflect how humans do or should reason about evidence; using it can mislead us.

One of the temptations of the mathematical account of probability is the illusion of precision. Numbers are impressively powerful in a great many contexts. Trying to apply numbers to every context, however, is something like trying to treat every subject as a science. Scientism is a mistake; so is thinking that all probabilities are numerical. We should expect only the degree of precision appropriate to the subject matter.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

CONCLUSION OF CHAPTER 5

This chapter has argued that despite the trend in recent work on evidential probability, Bayesians do not have a lock on the field. The Dutch Book argument shows that when the assumption that numerical degrees of probability apply is correct, then the axioms of mathematical probability theory are indeed conditions for reasonable belief. However, not many situations outside of artificial games of chance are clearly cases where numerical degrees of belief do apply. In fact, many of the most important situations in which we want to do evidential reasoning—in science, law, and ordinary life—seem not to be ones in which numerical probability can be made to work. The existence of the Dutch Book argument does not prove that Hume must not employ the non-numerical theory of probability in his case against miracles. These considerations, plus the fact that there are so many competing and mutually contradictory Bayesian analyses of Hume's argument against miracles, show clearly that it is a mistake to blame Hume for not giving a Bayesian analysis of the evidence for and against believing in miracles.

My view is that we can expand Gillies's pluralism about probability to include not just differing interpretations of mathematical probability in different kinds of situations, but also to include room for non-numerical probabilistic analyses in many kinds of cases involving empirical evidence and probable judgments. (This is consistent, too, with the sort of pluralism about science and philosophy of science advocated by Ruphy [2011].) And while subjectivist Bayesians insist that, in order to be considered rational, degrees of belief must be bound by two constraints, conformity with the axioms of mathematical probability at a moment and updating over time by conditionalization on new evidence (Earman 2002, 104), recent evidence from experimental psychology suggests that real humans do not in fact update their degrees of belief through Bayesian conditionalization (Mandelbaum 2018). While the conclusion that humans are not fully rational creatures is one that Hume would be comfortable with, I rather suspect that on this question he would have ultimately judged the Bayesian assumptions to simply be bad criteria for the kinds of cases he was concerned with.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

SIX

Resolving an Apparent Tension within Hume's Epistemology

ARE HUME'S NATURALISM, NORMATIVISM, AND VOLUNTARISM INCONSISTENT?

Several aspects of Hume's argument against believing in miracles entail a form of epistemic voluntarism—the view that what we believe is a matter of our own free choice. One who denies this sort of epistemic voluntarism holds that beliefs are beyond our voluntary control, that they "just happen to us" in the way that sensations do.

Along with this epistemic voluntarism comes a quite strong epistemic normativism—the view that in a given evidential context it would be correct to hold some beliefs and incorrect to hold others, and even that it is correct to hold some beliefs with a particular degree of confidence, rather than with any other degree of belief, in a given evidential situation.

The most straightforward instance of this normativism is Hume's injunction that "a wise man, therefore, proportions his belief to the evidence" (EHU 10.4). This entails that there is a correct degree of belief, or perhaps a small range of acceptable degrees of belief, for any given empirical conclusion relative to a body of available evidence. Clearly, this is a normative claim; Hume's is not an "anything goes" or "whatever happens" epistemic position, despite the fact that he challenges the rational foundations of inductive and even of demonstrative reasoning. Moreover, Hume's use of the active voice ("a wise man *proportions* his belief") clearly suggests voluntary control over the degree of belief one attaches to a proposition. This is something we (can) do to our degree of belief in (at least some) propositions.

George fails to correctly account for Hume's normativism when he supposes that "it seems open to [Hume] to have held that a belief of ours

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

need not be rejected as irrational simply because we lack reasons for it" (2016, 67). On my reading, Hume would definitely repudiate George's claim here. Hume does require adequate reasons for rational belief; a lack of reasons is a lack of adequate reasons, and hence a belief with no reasons is irrational (or at least arational) for Hume. There are plenty of things which Hume accepts as being simply part of our psychological makeup for which we have no reasons (for example, the expectation that the future will resemble the past, or the principles of the association of ideas), but Hume's point is that these are things which are irrational parts of human nature.

We see both the voluntarism and the normativism in this description of Hume's fundamental position on probability:

> Hume draws some basic distinctions in the *Treatise* that plainly indicate that he does not believe that all our inductive inferences are equally unjustified. He points out, for example, that philosophers distinguish "unphilosophical probability" from our reasoning based on the probability of chances and of causes. The latter forms of probability are "allow'd to be reasonable foundations of belief and opinion," whereas the former "have not had the good fortune to obtain the same sanction" [THN 1.3.13.1]. In the case of unphilosophical probability the operations of the imagination influence belief in ways that we cannot reflectively endorse. (Russell 2004, 440-441)

There is a potential problem with the combination of Hume's voluntarism and normativism with his naturalism, however. Asserting that we have voluntary control over the degree of vivacity of a belief goes against Hume's claims elsewhere about what we might call the purely psychological (that is, non-rational) mechanisms governing the production of vivacity, namely those principles Hume calls customs or habits of the mind. For example, there is an important sense in which for Hume *no* inductive belief is rationally justified, despite the quotation just given: "Hume acknowledged that we are psychologically constrained to reason from available evidence to conclusions such as that the Sun rises, that fire burns, that water drowns, etc.; so indeed might any reasonable person be constrained. Nevertheless, we are not thereby shown to be rationally justified in reasoning from that evidence to those conclusions" (Gower 1997, 95).

If no inductive belief is rationally justified, as Hume argues in (EHU 4, especially Part 2), how can Hume consistently claim that belief in the occurrence of miracles has less justification than does non-belief given the available evidence? And if we cannot choose what we believe because we are creatures of habit, how can there be any normative judgment about what would be un/warranted to believe? Why would *evidence* matter to degree of belief at all?

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

The issue boils down to this: Is Hume really entitled to either his voluntarism or his normativism, given his other commitments? There are at least two aspects of Hume's philosophy that appear at first to be incompatible with his epistemic voluntarism and epistemic normativism. First, if Hume is not entitled to his normative claims, he is not justified in saying that belief in miracles is epistemically inappropriate. And second, if epistemic voluntarism is inconsistent with Hume's other philosophical commitments regarding the role of custom and habit in belief formation, then he is not justified in blaming anyone for failing to meet the normative standard. I think the apparent inconsistencies here can be resolved, and Hume's position shown to be consistent and well supported, but the challenge is serious and deserves careful consideration.

The challenge I am concerned with is somewhat related to the "skeptical crisis" Fogelin (2009) attempts to resolve, namely, how Hume can hold to his desire to "introduce the experimental method of reasoning into moral subjects" (as he puts it in the subtitle of the *Treatise of Human Nature*) when early in his investigation he discovers that the very methods of experimental reasoning are rationally unfounded and based on a mere psychological expectation or habit of the mind. Fogelin (2009) traces Hume's dialectical path from radical skepticism back to a mitigated, moderate skepticism. As he says, "When matters are placed on an argumentative basis, the Pyrrhonist [i.e., the radical skeptic] always wins. For Hume, the slide into radical skepticism can only be countered by yielding in some measure to our vulgar [i.e., ordinary/non-philosophical] propensity to believe things that are not based on sound arguments and, more deeply, even things that run counter to sound arguments" (Fogelin 2009, 7). Thus, for Hume the science of human nature tells us how we do in fact form beliefs and it removes the pretense to reason with which we commonly puff ourselves up in many areas of life. We will see that Hume's move toward mitigated skepticism, based on recognition of how humans do in fact form beliefs, is a key part, too, of resolving the apparent conflict between his voluntarism, normativism, and naturalism.

FIRST STEPS TO A RESOLUTION

The first of the two worries I will address has its source in the fact that Hume is supposed to be a radical inductive skeptic, someone who doubts or denies our ability to make well-founded causal inferences, inductive inferences—and even (in "Of Skepticism with Regard to Reason," THN 1.4.1) demonstrative inferences. How can a radical skeptic like this claim that there is a right thing and a wrong thing to believe in any given case? The second worry turns out to be closely related to the first, and arises from Hume's fundamental epistemological principles, according to which degree of belief does not affect what today we might call the

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

"propositional content" of the belief but rather merely affects the *vivacity* (intensity) of the belief. On Hume's account, increases and decreases in the vivacity of ideas occur through a collection of non-rational, non-voluntary features of experience and human cognitive psychology. These include the "relations" of resemblance, contiguity in time and space, repetition, and so forth (see, for example, THN 1.1.5). How can it be that Hume analyzes degree of vivacity in these terms and yet says that we *ought* to have a certain (very low) level of vivacity in our beliefs concerning miracles? Shouldn't Hume be committed to the view that whatever we *do* in fact believe (as a result of these non-voluntary processes) is what we *must* believe? "[I]t seems that Hume is inconsistent in treating beliefs in miracles as worse than other beliefs about matters of fact that go beyond present experience and memory" (Garrett 2002, 307-08).

In the *Treatise*, Hume himself recognizes the paradox apparent in his view, and suggests a way to resolve it:

> According to my system, all reasonings are nothing but the effects of custom; and custom has no influence, but by enlivening the imagination, and giving us a strong conception of any object. It may, therefore, be concluded, that our judgment and imagination can never be contrary, and that custom cannot operate on the latter faculty after such a manner, as to render it opposite to the former. This difficulty we can remove after no other manner, than by supposing the influence of general rules. (THN 1.3.13.11)

The fallibility of deduction discussed in "Of scepticism with regard to reason" comes in at the level of the *application* of the rules of deductive inference. The rules themselves, Hume says, are "certain and infallible" (THN 1.4.1.1). It is not that geometry or arithmetic are merely probable or uncertain as sciences, but that we human beings are not very good at applying those rules of reasoning.

In a later section entitled "Rules by which to judge of causes and effects" (THN 1.3.15), Hume supplies a set of general rules for reasoning about empirical matters of fact. Before telling us just what those rules are, however, Hume notes a difference between the vulgar and the wise with regard to how general rules are employed in actual reasoning:

> When an object appears, that resembles any cause in very considerable circumstances, the imagination naturally carries us to a lively conception of the usual effect, tho' the object be different in the most material and most efficacious circumstances from that cause. Here is the first influence of general rules. But when we take a review of this act of the mind, and compare it with the more general and authentic operations of the understanding, we find it to be of an irregular nature, and destructive of all the most establish'd principles of reasoning; which is the cause of our rejecting it. This is the second influence of general rules, and implies the condemnation of the former. Sometimes the one, sometimes the other prevails, according to the disposition and character of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> the person. The vulgar are commonly guided by the first, and wise men by the second. . . . The following of general rules is a very unphilosophical species of probability; and yet 'tis only by following them that we can correct this, and all other unphilosophical probabilities. (THN 1.3.13.12)

Although this passage is a little hard to follow, the upshot seems to be this: Someone who reasons from a mere *prima facie* resemblance may reach a conclusion that is different from that reached by someone who reasons from a deeper analysis of the similarities and differences between the present case and past cases. Both people are using or following general rules. In both cases the general rule employed is that past observations are a guide to future observations; that is, we are doing inductive projection based on custom. The difference is that in the second case a more careful analysis of the present conditions has been made (using "the more general and authentic operations of the understanding"), and the relative weights of both the similarities and differences are given proper consideration in the inductive projection. This more careful, more detailed analysis is a better use of our powers of reasoning, limited though those powers may be and despite the fact that we cannot give a rational justification of inductive reasoning in general.

To make a connection to the medieval background that has played an important role in this book,

> It is worth mentioning that most of Hume's Rules [by which to judge of causes and effects, THU 1.3.15] were familiar to such medieval writers' as Scotus, Ockham, Grosseteste and Nicholaus of Autrecourt. The first four rules—continguity [sic] in space and time, priority of cause, constant conjunction, and same cause, same effect—were medieval commonplaces. The remaining four are to be found, separately, in the works of various writers, Ockham, Grosseteste, Buridan and Nicholaus among them. (Brown 1987, 665)

While it is true that Hume is not the innovator of all the rules of reasoning he invokes nor the first to espouse many of the positions he takes—in philosophy, who is, really?—he does package his ideas in a coherent way, relying on good principles accepted by his predecessors and contemporaries, and makes a solid case for the fundamental underpinnings of his reasoning as well as his conclusions. Hume's immersion in the ancient and medieval tradition of reasoning about evidence is a strength of his account, since that tradition stood up to the test of time.

WHENCE HUME'S NORMATIVISM?

Up to this point in this chapter I have argued that Hume's skepticism is mitigated by his recognition that there is after all something like a foun-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

dation (though not a rational foundation) for certain kinds of beliefs. As Russell says,

> [I]f we read Hume as a radical sceptic on this subject we are mistaking his starting point for his final destination. Hume begins by noting that reason cannot serve as the foundation for our inferences based on experience, but he moves on to show that the actual foundation of these inferences rests with the principles of association that facilitate the transition among our ideas and generate the conditions of belief on which human life entirely depends. Hume's concern, therefore, is not so much to show that all probable reasoning lacks any "rational justification," as it is to show that this form of reasoning depends on the activity and operations of the imagination. It is custom, not reason, that is the foundation of the inferences that we make and that serves as our "great guide in life." The distinction that we make between reasonable and unreasonable inductive beliefs is one that itself rests on the natural foundations of custom. (Russell 2004, 441-442)

The preceding discussion of the better uses of our powers of reasoning does not quite take us far enough, however. Why should a "more thorough" application of induction based on custom be the *better* way of reasoning? And how are we to decide what counts as a "more general and authentic operation of the understanding"? It seems Hume might just be begging the question by calling one "vulgar" and the other "wise." If inductive reasoning concerning matters of fact itself cannot be rationally justified, how can drawing a distinction between better and worse kinds of inductive reasoning be justified? Why attempt to draw such a distinction at all? The basic answer is that Hume is not really a radical relativist or a radical skeptic, despite initial appearances to the contrary. He wants to be able to show that some kinds of reasoning from evidence *are* epistemically superior to others.

> When Hume argues that the thesis of the uniformity of nature cannot be "produced by any argument or process of the understanding," he is not thereby denying or doubting the thesis of the uniformity of nature, nor passing a negative epistemic judgment on inductive inferences from experience. Rather, he is making a claim in cognitive psychology about the causal origin of such inferences: namely, that it is not the result of any prior or intermediary argument or operation of our inferential faculty that we are caused to engage in them. . . . [Hume] holds that he, like every other human being, *must* perform such inferences and accept the thesis [of the uniformity of nature] in practice, given his and our shared inductive cognitive mechanisms. (Garrett 2002, 326-27)

Hume thus draws the distinction between better and worse kinds of evidential reasoning in terms of *consistency*. He identifies the fundamental way in which we reason from past cases to future cases (the experience of constant conjunction in past experience leads us by a habit of the mind to expect future cases to resemble past cases), and he argues that

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

the more consistent application of this pattern of reasoning gives the better results. So, what is the basis for the judgment that the more consistent application is better? It is simply another inductive projection: We have found in past experience that the more careful, more consistent reasoner is more often correct, and we therefore cannot help but expect that this strategy will continue to be the more effective one in the future.

Above I quoted Hume as saying, "The following of general rules is a very unphilosophical species of probability; and yet 'tis only by following them that we can correct this, and all other unphilosophical probabilities" (THN 1.3.13.12). Why is it that following rules leads to (or is) an "unphilosophical" species of probability? For Hume, unphilosophical probabilities are probabilities (that is, degrees of belief, understood in his system as degrees of vivacity of beliefs) arrived at in ways that are less than solid. He gives examples in the early paragraphs of THN 1.3.13 such as the following: The distance of a memory, or the fact that something is partially forgotten, may diminish the degree of vivacity of some belief inferred from the memory. This is a psychological fact, but the probability of the proposition derived from the half-forgotten memory is not really lower simply because our memory is weak: The drunkard "who has seen his companion die of a debauch" (THN 1.3.13.2) should still be just as afraid of falling to the same fate a year later as he was the day after his friend's death. A more consistent application of general rules of induction would correct our drunkard's mistake.

Hume remarks that "A *fourth* unphilosophical species of probability is that deriv'd from *general rules*, which we rashly form to ourselves, and which are the source of what we properly call PREJUDICE. . . . Human nature is very subject to errors of this kind" (THN 1.3.13.7). Hume's examples are the general rules, "That an *Irishman* cannot have wit, and a *Frenchman* cannot have solidity" (THN 1.3.13.7). Clearly these are prejudices that are founded on false or hasty generalizations. They are, in other words, general rules which inform the probabilities that we attach to various beliefs, but the rules themselves are faulty. So when Hume says that, "The following of general rules is a very unphilosophical species of probability; and yet 'tis only by following them that we can correct this, and all other unphilosophical probabilities" (THN 1.3.13.12), what he means is that we can correct our reasoning that is based on false generalizations by making more careful use of general rules and inductive reasoning—that is, we correct our errors of reasoning by living up to the principle that our degree of belief should conform to the available evidence.

What makes it so that *the prejudice* has inadequate evidence? Why not say that the prejudice is acceptable and the results of other potential rules unacceptable? I think that what Hume has in mind here is that if there exists any previously observed connection between lack of intelligence and Irishness (for example), it is merely the result of an accidental regu-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

larity of experience. That is, it is a correlation that seemed regular on limited evidence, but which does not actually stand up to detailed scrutiny. (In modern statistical terms, we could possibly explain the generalization about Irishmen as having arisen from a sampling error or selection bias.) More extensive, honest, and careful observations will uncover Irishmen with wit, and thus destroy the prejudice. Thus, by careful use of inductive reasoning, in which we form probabilities based on the true proportions within the evidence rather than based on the frequencies within skewed samples, we can correct our mistaken general rules. If the world is really regular, this is the best way of reasoning in that it maximizes the chances of inductive success.

In addition to the problem of justifying Hume's distinction between vulgar and wise uses of general rules, there is another problem. How is it, on Hume's analysis of the role habits of thought in reasoning about matters of fact, that it is even possible for two different people to reason to two different conclusions given the same background experience and the same present observations? On the one hand, Hume is saying that empirical beliefs arise naturally and unavoidably, in a way that is governed by our inherent and inescapable psychological makeup (and which we cannot rationally justify). On the other hand, Hume is saying that we have voluntary control over which conclusions we accept. In short, Hume's voluntarism of belief is at least in tension with—if it is not in outright contradiction with—his account of the inductive generation of beliefs about matters of fact that are not directly observed.

Hume's position on the credibility of inductive conclusions is complex and nuanced. On one hand, Hume says that strictly speaking such conclusions are always rationally unjustified, because we can never be sure that the future will resemble the past. On the other hand, Hume takes his psychologically-based description of inductive belief formation (including the role of habits of thought) to be normative. As in the case of miracles, Hume often wants to legislate how a reasonable person *ought* to reason about the evidence—thus, when two people draw different conclusions from the same background experiences and observations, at least one of them is wrong. But given that he thinks that our inductive behavior is rationally unjustified, it is difficult to see how Hume's normative impulses can be legitimate.

The apparent conflict here is actually resolved by Hume's admission that we cannot be radical skeptics in life, but only in our philosophical moments. In ordinary life, we unavoidably reason like ordinary people, not philosophical skeptics: We reason on the basis of past experience that fire will warm us and bread nourish us—having studied some philosophy merely makes us less dogmatic about those beliefs. Judgments about the irrationality of induction and about the irrationality of belief in miracles are at different (and incommensurable) levels of analysis. Hume might put it this way: We cannot help but assent to inductive conclusions

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

in the course of ordinary life—doing so, as Hume says, is "a species of natural instinct" (EHU 5.1.8)—even though we can give no rational justification for our inductive conclusions when reasoning philosophically.

We cannot help but reason about matters of fact from the expectation that the future will resemble the past. And, *within the sphere of ordinary life* there are better and worse ways to reason about matters of fact. Perhaps it is not far wrong to say that the essence of the normative dimension of Hume's epistemology is *consistency*. If we all do (and must unavoidably) reason in a given way about simple inductive cases such as the nourishment derived from bread, then we should, on pain of inconsistency, reason in the same way about all general matters of fact. Hume shows that our ordinary inductive principles apply to the case of evaluating the evidence for miracles. Accepting the occurrence of a miracle would thus involve perversely abandoning the only kind of reasoning about evidence that has any claim to ordinary rationality—however weak that claim to rationality might ultimately be at the level of abstruse philosophical analysis. In short, Hume's inductive skepticism is in fact irrelevant to his account of reasoning about the evidence for miracles.

At THN 1.4.1.7-9, Hume resolves the skeptical doubts concerning demonstrative reason. (See also EHU 5.) His point there is that if belief or judgment were a matter of purely rational thought, we would end up in total skepticism; we don't, so, it isn't. Rather, belief and judgment depend in part on a manner of conceiving—that is, they depend on the force and vivacity of belief. It follows then that "belief is more properly an act of the sensitive, than of the cogitative part of our natures" (THN 1.4.1.8). Later, Hume says it even more directly: "reasoning and belief is some sensation or peculiar manner of conception, which 'tis impossible for mere ideas and reflections to destroy" (THN 1.4.1.8). In ordinary life, then, we unavoidably reason in accordance with the psychological principles of association that Hume identifies—resemblance, contiguity in space or time, cause and effect—even though it is impossible to rationally justify those principles at the level of philosophical analysis. These principles of association are the heart of human reasoning, and they are irreducibly the products of the kind of mind we have, the kind of animal we are. All reasoning concerning matters of fact depends on the Principle of the Uniformity of Nature, which says that the future will resemble the past, and it is impossible to prove that this principle is true. It is, instead, a habit of the mind that we cannot help but employ. Today, we might say it functions like a regulative ideal or a maxim of reasoning.

In the *Enquiry concerning Human Understanding*, Hume summarizes his point this way: "it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes, which are, from appearance, similar" (EHU 4.2.23). Although Hume immediately follows this up with the modest claim, "If I be right, I pretend not to

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

have made any mighty discovery" (EHU 4.2.23); in my view, this move is Hume's most profound contribution to epistemology.

George (2016, 64–68) raises a version of an objection to Hume's argument against miracles that is fairly common in the pro-miracle literature (George himself mentions finding the argument in C.D. Broad 1916–17), namely an argument to the effect that since Hume is an inductive skeptic, he is not entitled to the claim that the evidence for the laws of nature is stronger than the evidence that a law violation has occurred. As George points out, such an objection really proves too much:

> One might put the point this way: either past experience does furnish us with a proof of certain lawlike claims, or it does not. If it does, then Hume's argument against the rationality of belief in miracles stands. If it does not, then such a belief still cannot be rationally defended for it is premised on the evidential force of testimony, which (Hume insists) largely vanishes if past correlations between testimony and reality tell us nothing about whether such a correlation holds in general. (George, 65)

In other words, if inductive reasoning is radically unjustified and therefore cannot prove the laws of nature, neither can we get evidence about the occurrence of a miracle from testimony. The evidential value of testimony arises precisely because of a regularity of past experience in which testimony under certain kinds of circumstances has or has not been reliable. Given the impossibility of justifying induction, there is then no rationally justified empirical knowledge at all, and (*a fortiori*) no rational evidence for miracles. This objection defeats Hume's argument against miracles at the cost of making every kind of rational belief about matters of fact impossible. Hume obviously thinks we can and do form rational beliefs about matters of fact, so this radical skeptical interpretation cannot succeed and the objection to the argument against miracles based on this interpretation therefore clearly fails.

The correct response to this sort of objection is to point out that Hume's inductive skepticism applies only at the level of philosophical analysis, not at the level of ordinary life, as discussed above. So it is true, for Hume, that no empirical beliefs are rationally justified at the level of philosophical analysis, but at the level of ordinary analysis there are clearly better and worse ways to reason about matters of fact—ways determined by conformity with the ways the basic principles of human nature operate, as revealed by experience. Judgments about the rationality of induction and the rationality of beliefs about the occurrence of miracles are at different (and incommensurable) levels of analysis from one another. In ordinary life, we cannot but accept inductive reasoning since the expectation that the future will resemble the past is "a species of natural instinct" (EHU 5.1.8), which is to say that it is *unavoidable for us* even though we cannot rationally justify this use of the Principle of the

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Uniformity of Nature. When *using* induction, we will judge that belief in the occurrence of miracles is incorrect, *on pain of inconsistency with the fundamental operations of our minds*. To put the point another way, belief in miracles is only possible by abandoning the essence of how humans know matters of fact. It is a belief in miracles that undermines *ordinary* rationality, not the inductive skepticism operative at the *philosophical* level.

One might challenge this move by asking for an explanation of why consistency in our reasoning about matters of fact is required. Hume's answer would simply be that his account of reasoning about matters of fact is just a description of how we *do* reason. Anything else is not only (like any other principle we might consider) unjustified at the philosophical level, but also unfounded in human nature at the ordinary level of analysis. In ordinary life, we experience better and worse uses of reasoning about evidence, and through careful attention to this experience we form normative standards for the proper conduct of probable reasoning. We must then apply those standards consistently across all our reasonings concerning matters of fact, including the case of purported miracles, or else *by our own lights* we will be not be reasoning in the best way possible. This is, similarly, the upshot of Hume's remarks on unphilosophical probability, namely, regarding the human tendency to draw certain kinds of incorrect conclusions about probable beliefs: Those sorts of errors can be corrected by deriving rules of reasoning from careful attention to cases where reasoning about probability goes well (e.g., in the case of the evidence for the laws of nature) and those where it goes poorly (e.g., in the case of the evidence for miracles).

Now, it is important to point out that Hume's skepticism—its extent, manner, target, and relative success—has been a perennial topic of philosophical debate, and most worryingly has been the subject of several fads of interpretation that seem to have more to do with the history and viewpoint of the interpreters' eras rather than with true innovations that pushed objective interpretation in one direction or another. Admittedly, Hume's position is subtle and complex, and it isn't one of the "standard" positions one would expect in epistemology, so it is little wonder, then, that the preoccupations of various philosophical schools have caused their adherents to read Hume through their own lenses. And, fair enough: How could that be avoided? Still, it seems difficult to square some interpretations with what's simply on the page, especially when the entirety of Hume's project is taken into account.

One main question seems to be, is Hume a radical skeptic? Historically, this was a common reading of Hume, until the naturalist interpretation of his epistemology gained traction in the middle of the twentieth century. By "radical skeptic" here one might mean various extreme positions to the effect that humans do not, or cannot, have knowledge—where that might be a generalized claim about all knowledge or a local-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

ized one about various kinds or sources of knowledge (perception, memory, deduction, induction, and so on). My answer is that Hume is clearly not a radical skeptic, though there is an important sense in which his philosophical project involves a journey through those dark and dangerous lands.

As Kevin Meeker writes in his *Hume's Radical Scepticism and the Fate of Naturalized Epistemology*, "The key difference between the sceptical reading and the naturalist reading centres, then, not on some new-found texts or some differing emphasis on preceding philosophers but, rather, on the argumentative support of certain key *texts.* That is, the ascendency of the naturalist interpretations is due to the undermining of the arguments for the skeptical reading of Hume's texts, as well as textual evidence that has been marshalled for the naturalist reading" (Meeker 2013, 3).

Contrary to the naturalistic turn in Humean scholarship, Meeker argues that Hume is indeed a radical skeptic. Meeker's full account is not simplistic, however, since he distinguishes several varieties of skepticism and locates Hume within each. He concludes that Hume is a skeptic in the sense of thinking that no belief is certain and that all beliefs "lack the positive epistemic status of being more justified/rational/warranted than their contraries" (Meeker 2013, 17). At the same time, Meeker argues that Hume is not someone who thinks that all beliefs should be abandoned or that we should adopt a generalized doubt and suspend judgment regarding all beliefs.

Describing the details of Meeker's argument for this reading of Hume would take us too far afield here. It will have to suffice to note that Meeker compares his approach to prominent naturalist accounts of Hume and marshals textual and argumentative support for his view. Interestingly, he argues that all versions of naturalized epistemology must inevitably lead to radical skepticism.

Where Meeker and I diverge is mostly on this fault line. Whereas I agree that Hume argues that we have no knowledge in the strict sense of the word from perception, demonstration, or induction, I disagree that Hume stops there. Hume's point, in my view, is to show that at the level of philosophical investigation, there is no "ultimate" justification of human knowledge possible. This is to say simply that the foundational systems of the philosophers are of no use to us in grounding human understanding. Part of Hume's goal, of course, is to convince us, therefore, that we should put aside all previous approaches to epistemology and pursue Hume's own science of human nature. (Our contemporary cognitive psychology, somewhat amazingly, largely supports Hume's associationist epistemology of belief.) But then, in a way that seems confusing or false to those with a more typical approach to epistemology than Hume, Hume entirely puts aside his skeptical conclusions (except with the goal of promoting the lingering epistemic humility of knowing that, really, we know nothing), and he investigates the methods of proper

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

reasoning and belief formation in ordinary life. There, he concludes there are better and worse ways of reasoning, and beliefs that are more justified than their contraries, despite the fact that at the philosophical level he has concluded that beliefs cannot be justified.

In his book, *Hume's True Scepticism*, Donald Ainslie considers in detail Hume's skepticism about a whole slew of topics including the external world, personhood, substances, sensations—and, of course, both demonstrative and probable reasoning. In articulating his interpretation of Hume's position on these matters, he writes,

> Hume allows for the legitimacy of the philosophical question of whether and how to trust our tendencies to believe. It turns out that his question cannot be answered using the only methods for philosophy he countenances. Thus he ends up a sceptic about what philosophy can accomplish, and also a sceptic who embraces his fundamental tendencies to believe blindly. Philosophy can neither justify nor undermine [those tendencies]. We move from false to true philosophy when we "return to the situation of the vulgar" (T[HN] 1.4.3.10, SBN 224) by relying on our reasoning, sensing, and introspecting, though now with an understanding of the situation. (Ainslie 2015, 243; his citation for the quotation is off by a paragraph: It is actually THN 1.4.3.9, SBN 223; by "vulgar" Hume means "ordinary" or "non-philosophical.")

One of Ainslie's nice points here is that "Philosophy can neither justify *nor undermine*" the natural tendencies of the mind to reason in particular ways, especially in using the expectation that the future will resemble the past in inductive projections from past experience. We may not be able to rationally justify probable reasoning, but neither can reason show probable reasoning to be completely unjustifiable. (That's a good thing, since induction is so very reliable.) Thus, Hume's position is that, at the level of philosophical inquiry, we are forced by good reasoning to adopt radical skepticism about most things, including our ability to reason about evidence; but, in ordinary life, we do and in fact must reason about matters of fact in accordance with these psychological principles about which we find ourselves in clouds of doubt:

> Most fortunately it happens, that since reason is incapable of dispelling these clouds, nature herself suffices to that purpose, and cures me of the philosophical melancholy and delirium, either by relaxing this bent of mind, or by some avocation, and lively impression of my senses, which obliterate all these chimeras. I dine, I play a game of backgammon, I converse, and am merry with my friends; and when after three or four hour's amusement, I wou'd return to these speculations, they appear so cold, and strain'd, and ridiculous, that I cannot find in my heart to enter into them any further. (THN 1.4.7.9)

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

DAVID OWEN ON HUME'S DEFENSE OF PROBABLE REASONING

Owen (1996) tackles three questions very similar to the ones addressed so far in this chapter. Discussing these will provide an opportunity to flesh out the account I am offering about why there is no incompatibility between Hume's epistemic voluntarism, normativism, and inductive skepticism. Owen lists his questions as follows: "(1) Given Hume's negative arguments about probable reason, what right does he have to say that we ought to proportion our beliefs to the evidence? (2) Given Hume's theory of belief, how is it even possible to proportion our beliefs? (3) Why should we proportion our beliefs according to the probable reason, rather than follow superstition and bigotry?" (Owen 1996, 489).

Owen answers his three questions in turn along the following lines. Hume's warrant for his normativity (Owen's question 1) comes from the fact that while we cannot help but use custom in simple cases of belief formation, after long experience we can reflect upon how we do form beliefs about matters of fact, and from this we formulate general rules (for example, about reasoning from causes to effects). While we are prone to errors such as over-hasty judgments and prejudices derived from insufficient evidence, these errors are

> themselves the result of an inappropriate use of general rules [and] can be corrected by reflecting on "the more general and authentic operations of the understanding," which recognizes the earlier sort of judgment to be "of an irregular nature, and destructive of all the most establish'd principles of reasoning. . . . The vulgar are more commonly guided by the first, and wise men by the second." (Owen 1996, 492; quoting THN 1.3.13.12; ellipsis in Owen)

As for his second question, Owen answers it by appealing to a distinction between the mechanism of belief formation, which we cannot alter, and the "input conditions" such as ideas, memories, and general rules, which we can vary (Owen 1996, 493). It is important to recognize, too, a distinction that Hume is careful to draw: "beliefs can be formed immediately (as in sense-perception and memory) and mediately (as the result of probable reasoning)" (Owen 1996, 492). The immediate beliefs we have no voluntary control over, the ones that are formed mediately can be modified as to content or vivacity by further mediate reasoning and reflection. So, while it is true that for Hume our beliefs about matters of fact, "are proportioned for us, according to past experience," nevertheless, "we can make sure that our past experience is inclusive of the relevant sorts of events. We can make sure that we concentrate on all the relevant circumstances, and not ignore relevant evidence. We can make use of general rules. We can reflect on past errors. We can check our memory against documentation. In short, we can vary the context in which beliefs arise" (Owen 1996, 493). It is interesting to note that these are just the sorts of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

techniques that might be used in a court of law to make sure that the correct judgments get made. Perhaps this is another connection between Hume and the tradition of reasoning about probability that derives from ancient Roman law.

Let me pause for a moment in my description of Owen's position. One could legitimately challenge Hume on the account as so far described by pointing out that for those who are direct witnesses to a miracle, the idea is formed immediately through an impression of sensation, not mediately through testimony, imagination, or reasoning. Someone *sees* the miracle event occur (or thinks they do, which for present purposes is equivalent since the impression provides an immediate idea in either case): Water turns into wine, a lame person walks, a blind person sees, someone rises from the dead, someone walks on water, a statue of Mary weeps blood, a traffic accident involving a tile delivery truck leads to a reproduction of The Last Supper laying on the Pennsylvania Turnpike in which the figure of Jesus looks just like Elvis. As a direct witness, the idea we form of each of these events comes directly from experience and is not mediated. A good Humean answer to this challenge, I think, is to say that even though the direct perception of the event means that the idea of the event is immediate and not susceptible to voluntary control, nevertheless the interpretation of the event *as a violation of a law of nature* does not come directly from perception. It may be a hidden and unconscious inference, but it is nevertheless an inferential leap, and thus the premises of the hidden mediate argument can be vetted by reason. We do not directly see laws of nature either; they are mediately constructed in our minds as a result of unavoidable psychological tendencies acting upon regularities of experience. We cannot directly see an event *as* a law violation because we cannot see the causes underlying the event, and thus we cannot be sure that there was not some misperception, some interfering cause, or some previously unknown law in play.

To return to the description of Owen's position: About his third question, Owen says, "we are currently concerned with the question not of what philosophers should do, but of whether we should be philosophers" (Owen 1996, 495). The vulgar follow weaker principles of reasoning, whereas philosophers "refine it into something more sophisticated" (495); but these are the same sophisticated principles that lead to skeptical doubts regarding reason, and if those are in force Hume is not entitled to his normativity. Owen's answer is nuanced and complex and longer than I can give full treatment of here, but its essence is that Hume argues that a skeptical philosophical approach to the world is safer and more agreeable than the arrogant and dogmatic vulgar way of being.

> The sceptical philosopher will temper his reliance on reason, but will not, on that account, fly whichever way his natural sentiments incline him. "A true sceptic will be diffident of his philosophical doubts, as

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

> well as of his philosophical conviction" [THN 1.4.7.17]. The truly sceptical philosopher, espousing reason, but not dogmatically, following his inclinations, but not slavishly, is a proper object of moral approval. (Owen 1996, 498)

One way to navigate this tangle is to remember that even if we have skeptical doubts about probable reason, we cannot help but use probable reason in our ordinary lives. We do, unavoidably, reason inductively. And although we are unable to justify that practice and show *why* it is successful, it normally *is* very successful, especially when we are careful. Induction, though unjustified, is nevertheless reliable. (Call this reliability the "mystery" of induction as distinct from the more commonly discussed "problem" of induction, which has to do with the impossibility of justifying inductive practices.) When we properly attend to the appropriate evidence and circumstances, we cannot help but notice, in the same manner that we notice constant conjunctions in experience, which patterns of sophisticated probable reasoning are successful more often than others, and that these sophisticated patterns of probable reasoning are much more effective than the vulgar ones. So, we cannot help but use induction, and unless we are to abandon "the very guide of life" we should and must continue to use the same kind of reasoning to study and correct our own probable reasoning, even of the sophisticated kind having to do with general rules. The truths Hume has noticed about the "foundations" of human reason are the very ones that, on pain of inconsistency, are to guide us when we reason using general rules. This is admittedly different than a foundationalist justification or vindication of the normativity of the general rules of probable reasoning that undergird the argument against miracles—but given Hume's basic epistemology we should never have expected a foundationalist justification or vindication. Hume would be quite sure, of course, that no opponent could come up with a foundationalist justification or vindication of a *contrary* conclusion about belief in miracles, either.

CAN WE EVEN FORM A BELIEF ABOUT A MIRACLE?

Bayne (2007) poses an interesting question: Can the argument against miracles even get started? Is it even possible, according to Hume's theory of belief formation, to acquire a belief that a miracle, a violation of the laws of nature, has occurred? If it is impossible to acquire such a belief in the first place, Hume could have saved himself the trouble of writing "Of Miracles." Moreover, Bayne is concerned, if it is not possible in Hume's system to even form a belief that a miracle has occurred, then this might undermine Hume's entire epistemology—for surely, some people do apparently have beliefs that miracles have occurred and any epistemology that does not make this possible is thereby undercut. Clearly Hume

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

speaks as though people do have beliefs in miracles; Bayne is concerned with the question of whether or not Hume's epistemology really has the resources to explain how one could acquire a belief in a miracle.

This question is relevant to the voluntarism aspect of the present chapter because it seems as though Hume suggests that we can choose to assent or dissent to a claim about the occurrence of a miracle—that is, that we can (at least to some extent) voluntarily believe or disbelieve the miracle. Clearly this will be impossible if a belief in a miracle cannot even be formed.

Bayne runs through an account of what Hume says about belief formation via causation, education, passion, and testimony, and argues that according to strict Humean principles none of these methods can produce a belief that a miracle has occurred. Ultimately, Bayne suggests that Hume can rely on a distinction between beliefs and counterfeit beliefs and thereby avoid the threat to his entire epistemology:

> Hume tells us that although it may appear that this enlivening of our ideas raises their vivacity to a level greater than that of custom and experience and so produces belief, in reality "there is always something more forcible and real in its [custom's] actions, than in the fervours of poetry and eloquence" [THN 1.3.10.10]. The enlivening of our ideas here produces not belief, but only, as Hume puts it, the "mere phantom of belief" or a "counterfeit belief." In the case of miracles, we would say the same thing. That is, when we get the idea of the occurrence of a miracle, the passions of surprise, wonder, and astonishment are produced in us. These passions in turn enliven our idea of the occurrence of a miracle. Now, it may appear that this gives our idea of the miraculous occurrence enough force and vivacity to overcome that of custom and experience and thus produce belief, but just as "in the warmth of a poetical enthusiasm, a poet has a counterfeit belief" [THN 1.3.10.10], so a person gripped by the passion for the miraculous only has a counterfeit belief and not the real thing. (Bayne 2007, 23)

I find Bayne's solution intriguing but ultimately unsatisfying. For Hume, belief is supposed to be nothing but the force and vivacity attached to an idea; and the degree of force and vivacity is just the degree of probability of the idea. Hume's talk of "counterfeit beliefs" does not, on my reading, create a new epistemic category. Rather, it is shorthand for ideas that illicitly come to have or come to seem to have sufficient force and vivacity to be accepted (believed), when in fact the force and vivacity should be appropriately pruned ("proportioned") by the use of general rules and careful attention. Through this process we will come to see that the force and vivacity originally attached to the ideas were acquired incorrectly (for example, through passions, imagination, and so on).

Bayne's solution commits Hume to saying that people *think* they believe some things but are mistaken about that; they do not actually believe what they think they believe. Since Hume, as far as I can tell, never

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

says anything like this, I would rather not have to invoke this way of speaking.

Of course we can form an idea of a miracle, since whatever is conceivable is possible and we can conceive of a violation of a law of nature. We can conceive of a bar of lead that does not fall when released, to use one of Hume's examples. This idea can arise in several ways, but the obvious way is via imagination, which puts together ideas that are not found together in experience. Once we have the idea, then through the mental operations of association, imagination, and/or testimony, the idea acquires force and vivacity through the various psychological mechanisms Hume explains (wonder, awe, surprise, emotion due to religious enthusiasm, the effect of testimony, repetition, etc.). In some cases, the idea may well acquire sufficient force and vivacity to rise to the level of assent. But careful reasoners, the wise and learned, will proportion their degree of belief in the idea of a miracle by consciously and carefully comparing the quality of the actual evidence for and against the occurrence of the supposed miracle. This rather simple story is all Hume needs to explain how belief in a miracle can arise within his epistemology. Bayne's problem is not a problem after all.

CONCLUSION OF CHAPTER 6

In this chapter I have offered a plausible solution to the apparent contradiction between Hume's emphasis on the role of custom in belief formation, his epistemic normativism, and his epistemic voluntarism. Hume is entitled to all three because his skeptical challenge to epistemic foundationalism, which results in his account of the formation of beliefs regarding empirical regularities through custom or habits of the mind, takes place at a level of philosophical analysis that cannot be sustained in ordinary life. In ordinary life, Hume insists, we must regulate and perfect the ordinary operations of the understanding so that we do as well as it is possible for humans to do when it comes to reasoning about evidence. This emphasis on reasoning consistently, in accordance with unavoidable principles of human cognition, is very different than a demonstration that human powers of reasoning are well-grounded. Since we can, in fact, reconsider evidence in this way, not only is this the right (and only) way for humans to regulate their beliefs, it is the normatively best way. Hume's shift to the level of ordinary analysis is thus the key to resolving the apparent conflict between his normativism, voluntarism, and naturalism.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

SEVEN

Flew, Fogelin, Ferguson, and Fogelin

FLEW AND FOGELIN ON THE A PRIORI IN HUME'S ARGUMENT AGAINST MIRACLES

In the years 1990-92, there was a vigorous debate in the pages of the journal *Hume Studies* about the correct interpretation of Hume on miracles. Robert Fogelin fired the first salvo, in part reacting against Antony Flew's influential book *Hume's Philosophy of Belief* (1961). Fogelin (1990) argues that the "traditional interpretation" of Hume on miracles is wrong, and that Flew is also wrong where he deviates from the traditional interpretation. I disagree with Fogelin (1990) on both counts.

According to Fogelin, the traditional interpretation says that,

> I. Hume did not put forward an a priori argument intended to show that miracles are not possible.
>
> II. Hume did put forward an a priori argument intended to show that testimony, however strong, could never make it reasonable to believe that a miracle had occurred. (Fogelin 1990, 81, citing Coleman 1988, 343, n. 4)

Flew (1961) agrees with *I* but denies *II*, that is, he argues that Hume's remarks on testimony do not amount to an a priori argument against the possibility that testimony could render believing in the occurrence of miracle reasonable. In this respect Flew's reading of Hume is consistent with my own, on which no part of Hume's argument rests on anything a priori. Fogelin's (1990) position is the opposite of Flew's: Fogelin argues that *I* is incorrect and *II* correct. The passages from "Of Miracles" to which he appeals, however, are replete with phrases that (to my mind at least) undermine his position. Fogelin argues that Hume was putting forward an a priori argument against the possibility of miracles occur-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

ring, and an a priori argument against the possibility of testimony ever being sufficient to warrant belief in the occurrence of a miracle. Fogelin's reconstruction of Hume's supposed a priori argument against the existence of a miracle is as follows. (This reconstruction has the appearance of circularity, but my impression is that Fogelin intends it to have a structure like that of modus tollens. It isn't really that, either, but that turns out to be the least of the problems.)

1. There is here a direct and full *proof*, from the nature of the fact, against the existence of any miracle; nor can such a proof be destroyed . . . but by an opposite proof, which is superior. [EHU 10.12]
2. The proof against a miracle, from the very nature of the fact, *is as entire as any argument from experience can possibly be imagined*. [EHU 10.12; emphasis added by Fogelin]

Therefore:

3. There is . . . a direct and full *proof*, from the nature of the fact, against the existence of any miracle. [EHU 10.12] (Fogelin 1990, 82-3)

Although Hume may, on first reading, seem to be talking in the passages cited about the ontological issue of whether or not miracles can possibly exist, in fact he is talking about *proofs* against the existence of miracles. Proof, as I have shown above, is for Hume an epistemological category—Humean proof has to do with credibility of evidence, not possibility of existence. Hume's claim in these passages is really that we cannot have sufficient evidence to *believe in* the existence of a miracle. Moreover, proof, as I have argued, is a probabilistic category. This means that Hume's argument is not an a priori one. It is, instead, empirical and thus a posteriori, as the phrase "argument from experience" in Fogelin's second premise should have made abundantly clear to him. Fogelin's reconstruction of the argument that he thinks shows that Hume was indeed giving an a priori argument against the possibility of testimony ever being sufficient to warrant belief in the occurrence of a miracle is flawed in a similar way. In short, Fogelin (1990) is wrong on both counts with regard to his theses *I* and *II* above. Neither part of Hume's argument regarding the existence or credibility of miracles is a priori. Fogelin (2003, 91, n. 8) now admits having misunderstood "proof" in Hume during the Flew/Fogelin debate in the 1990s.

Flew (1990) defends himself against Fogelin's charges and does so more or less along the lines of my account offered in this book. Flew does, however, make what I take to be a few significant mistakes of interpretation. Consider the following passage from Hume: "There must, therefore, be a uniform experience against every miraculous event, otherwise the event would not merit that appellation. And as a uniform experience

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

amounts to a proof, there is here a direct and full *proof*, from the nature of the fact, against the existence of any miracle; nor can such proof be destroyed, or the miracle rendered credible, but by an opposite proof, which is superior" (EHU 10.12).

Flew reads "from the nature of the fact" in this passage to mean "from the nature of the concept [of a miracle]." He writes that "What [Hume] is trying to demonstrate a priori in Part I [of "Of Miracles"] is: not that, as a matter of fact, miracles do not happen; but that, from the very nature of the concept—'from the very nature of the fact'—there must be a conflict of evidence required to show that they do" (Flew 1990, 141, quoting Flew [1961]). This is a serious misreading. Surely Hume, of all people, would not conflate facts with concepts? For Hume the phrase "from the very nature of the fact" signals the *empirical* character of his argument, whereas Flew's reading makes the impossibility of miracles a matter of *definition*. This only compounds the all-too-common error of interpreting Hume as giving an a priori or definitional argument against miracles.

Flew (1990) also fails to fully grasp what Hume means by "proof." Flew writes, "Certainly, to award to any argument the diploma title 'proof' is to imply that the conclusions proved thereby must be true" (Flew 1990, 143). This is imprecise: There is no guarantee that the conclusions of proofs are true, and on the receipt of further evidence we might come to think that the conclusion is false, even though the presently available evidence is so strong that we *treat* those conclusions *as if* they are true. The imprecision here leads Flew to claim something that is clearly false: "there cannot be contrary or contradictory proofs" (Flew 1990, 143). This would be correct if Humean proofs achieved 100 percent probability, but of course they do not. Proofs are based on evidence that leaves "no room for doubt" in the sense of giving us moral certainty. The phrase "no room for doubt" means no practical room for doubt, or no room for reasonable doubt. The independent evidence for two contrary propositions (propositions that cannot both be true together) might still provide no grounds for doubting either proposition, except for the opposing line of evidence. When we consider the opposing lines of evidence together, of course, the overall degree of assurance one way or another is going to be very low indeed (as in the case of the contrary religions argument discussed above, and in the example in the quotation in the next paragraph). Flew wants to read "proof" in this context as coming with a tacit qualification to the effect that the apparent proof is not really a proof, but such a move is unnecessary if we interpret Hume properly. Perhaps one way to understand Flew here is analogous to Hume's diminution principle (see above). Two opposing lines of evidence that *would have been* proofs taken each on their own, when combined destroy each other and leave no probability in either direction; whereas one of the lines of evidence would have left us with no room for doubt, both together force us to a perfect suspension of judgment.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Flew nevertheless does ultimately adopt what I think is the correct position, and he supports it with this important quotation from Hume: "Suppose . . . that the testimony [for a miracle] considered apart and in itself, amounts to an entire proof; in that case, there is proof [from testimony] against proof [from experience of natural laws], of which the strongest must prevail, but still with a diminution of its force, in proportion to that of its antagonist" (EHU 10.11). This is precisely the "diminution principle" that Earman attacks and Coleman defends. In the legal context, examples fitting this description—contests of proof against proof that produce no conviction—are easy to imagine. In a case where we have no other evidence except four unimpeachable witnesses, two for the defendant and two against, we have proof against proof (recall that two witnesses amount to a "full proof" in the legal tradition). We are in such cases in perfect doubt and must suspend judgment. We then acquit not because there is a preponderance of evidence of innocence, but because it is (ethically) safer to acquit the guilty than to convict the innocent: In the absence of decisive evidence, benefit of the doubt therefore goes to the accused.

Roman law had a rule that, "in cases of doubt, for example about the interpretation of wills, the more benign or humane interpretation was to be preferred as safer and more just. Another was that proofs of guilt in criminal cases be 'clearer than light.' Augustine asserted a principle of charitable interpretation, 'Doubts are to be interpreted in the better part'" (Franklin 2001, 66-7). Hume's diminution principle, I suggest, is a similar methodological rule, albeit a rule of epistemological as well as ethical safety. By Hume's lights, denying miracles is the safer route because it leaves us with a modest, circumspect skepticism about the world, whereas accepting miracles often leads people to dogmatically make and act on religious claims in ways that cause harm to others.

In the course of his argument, Flew also makes an interesting point about the fact that it would be futile for Hume to give an a priori argument for the logical or physical impossibility of miracles:

> Being unable to discover any antecedent impression from which the idea of physical necessity could be derived, Hume disqualifies himself from appealing thus openly and directly to the necessary physical impossibility of the miraculous. But, even if he had to his own satisfaction succeeded in legitimating that crucial concept, it would still have been pointless here to point to the (natural and) physical impossibility of the miraculous—as if this was a reason for thinking that there have not in fact been and could not conceivably have been or be Supernatural overridings of the natural order. (Flew 1990, 142)

The fact that an argument to establish the a priori impossibility of miracles would be futile is another good reason for thinking that Hume in fact

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

never offered such an argument. It would have established nothing of relevance to Hume's critique of religion.

FERGUSON'S INTERVENTION IN THE FLEW/FOGELIN DEBATE

Kenneth Ferguson (1992) intervenes in the Fogelin/Flew debate in part to make a claim similar to mine about the meaning of "proof" for Hume. He writes,

> By his own examples, namely, that "the sun will rise to-morrow" and "all men must dye" [THN 1.3.11.2] . . . , that "lead cannot, of itself, remain suspended in the air" and that "fire consumes wood" [EHU 10.12], Hume makes it clear that "proofs" are simply to be causal arguments uniformly confirmed by a great wealth of past experience: empirical conclusions so sound that any who deny them "wou'd appear ridiculous" [THN 1.3.11.2]. (Ferguson 1992, 106)

Ferguson also makes some important comments relevant to what was said above about singular events and singular apparent law violations: "Hume argues . . . to a proof that *every event has a cause*, and that, ultimately, every event will turn out to be causally determined (cf. also [THN 1.3.12.1-10])" (Ferguson 1992, 107). The claim here is that Hume has evidence from past experience that amounts to a *proof* that every event has a cause. The "causal principle" is thus not a priori but a posteriori for Hume, and is merely highly probable, not certain. If nothing else, the uniform past evidence for the causal principle does give sufficient weight to the methodological ideal according to which we should always try to find naturalistic explanations for apparent violations of the laws of nature. As Ferguson (1992, 107) argues, Hume thinks exceptions to laws should be attributed to "the secret operations of contrary [but natural] causes" (EHU 8.13).

Ferguson (1992, 108) goes on to try to show that Hume has a deductive (and hence a priori) argument against miracles, but the argument he constructs on Hume's behalf does not appear in Hume, and if the account offered in this book is correct, then Ferguson's argument violates basic Humean principles. Besides, an argument of the kind Ferguson constructs, one with an apparently deductive structure but probable premises (even highly probable premises such as "every event has a cause"), does not establish its conclusion with deductive certainty. This is to say that even if Ferguson's argument could be attributed to Hume, it would not amount (as he thinks, against Flew) to an a priori argument against believing in miracles. For this reason, I think that the ultimate conclusion Ferguson draws, namely that there is room for both Fogelin and Flew to be correct, cannot go through.

Ferguson's discussion (1992, 109) does bring out one other very interesting feature of Hume's argument, however. Suppose we have a well-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

established law (for example, the law that *people who attempt to walk on water, sink*), and we hear a report of an event that appears to violate that law (*someone has successfully walked on water*). In such a case Hume really has two options. The first is to disbelieve the occurrence of the event because we judge it likelier that the report is faulty due to misperception, deception, or mistransmission. The second is to allow that the *event* occurred but deny that it was really a violation of a law—that is, we allow that it occurred but assume that some naturalistic, law-based, albeit unknown explanation exists (for example, that the water-walker made use of magnetic-repulsion sandals or the like). This makes it clear that while Hume thinks it extremely unlikely that we would ever have grounds amounting to a proof to accept testimony about the occurrence of an apparent law violation, nevertheless even if we somehow did have grounds for accepting the testimony, we *still* would not have reason to think of that event as an actual violation of a law of nature. The conclusion against believing in miracles is thus rationally over-determined.

In the case of proof against proof—the evidence for a law versus impeccable testimony that there has been a violation of that law—Hume argues that a wise person who proportions her belief to the evidence will reject the miracle report in favor of protecting the law. Ferguson alleges that "there is no known theory of probability that would support such a rejection" (1992, 110). On the contrary, as shown earlier, while there is no interpretation of the *mathematical* theory of probability that would license the conclusion in question, the pre-Pascalian, non-mathematical theory of probability does support the rejection of the miracle in this sort of case.

To conclude my discussion of the Flew/Fogelin debate, let me mention just one of the many interesting points that Joseph Ellin (1993) makes while analyzing the Fogelin/Flew debate. Ellin notes that although Hume's opinion is that no miracle has ever occurred, "the arguments on both sides would be exactly the same even if there had been" (Ellin 1993, 209). That is, our evidence for the laws of nature, our assessment of the testimony about supposed law-violations, and the balancing of probabilities that comes out against miracles would be the same whether or not miracles have actually occurred. I think this gets Hume exactly right: It illustrates the fact that Hume distinguishes the metaphysical and epistemic questions about miracles, and it makes clear one of the reasons Hume is so sure that testimony to miracles will never be sufficient as the foundation for religious belief. This is "a priori" only in the very weak sense that it follows from what we have empirically discovered about how human beings do, in fact and unavoidably, reason about evidential probabilities. There is something like an "essence" of human cognitive psychology for Hume, though this is by no means definitional or logically necessary since it too is discovered through experience, including observation of our fellow humans and through introspection, and this can only be known to some degree of probability. Other beings with different

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

cognitive tendencies could well reason about evidence differently than humans do. Hume's point, seen through this lens, is that *we humans* cannot reasonably believe in the occurrence of miracles, given how we do in fact understand what "reasonable" means.

FOGELIN'S MORE RECENT MISREADINGS OF HUME

The draft of my original defense of Hume from Earman's attack had been submitted to *Hume Studies* in early 2004, before I had learned of the publication about six months before Fogelin's *A Defense of Hume on Miracles* (2003). Fortunately for me, even though Fogelin and I both sought to rebut Earman and provide a deeper understanding of what Hume really said and meant, our approaches were and are quite different. Fogelin's considered (2003) interpretation of Hume's argument against miracles (as opposed to Fogelin's 1990 interpretation) is in several points similar to mine. But his criticisms of Earman are in large part different from mine, and Fogelin actually attributes a proto-Bayesian theory of probability to Hume. Since the key part of my defense of Hume denies that Hume could countenance a mathematical approach to evidential probability, I must here engage Fogelin's work in detail and show where he goes wrong. As Richard Otte's (2005) review of Fogelin points out, the main text of Fogelin (2003) is only sixty-two pages long; it is really an extended essay with apparatus.

Fogelin responds to the bewildering array of differing interpretations of Hume's position on miracles by attempting to give a coherent reading of "Of Miracles." In doing so, he aims to correct what he sees as two common misreadings:

> The first misreading is that, in part 1 of his essay on miracles, Hume maintains that no testimony could ever be sufficient to establish the occurrence of a miracle. Hume does not say this in part 1. Indeed, Hume *nowhere* asserts this, though in part 2 he does say, "Upon the whole . . . it appears, that no testimony for any kind of miracle *has* ever amounted to a probability, much less to a proof" ([EHU 10.35; emphasis added by Fogelin]). The second common misreading of the text is that in part 1 Hume presents what he takes to be an a priori argument sufficient by itself to establish his fundamental theses concerning the status of testimony in behalf of miracles. This, I argue, is false. (Fogelin 2003, 2)

Fogelin then goes on to argue that the second common misreading is corrected by seeing part 2 of Hume's essay against miracles as an essential and not merely supplementary part of Hume's argument. It is worth pausing to note that the passage just quoted—"Upon the whole . . . it appears, that no testimony for any kind of miracle *has ever amounted* to a probability, much less to a proof"—makes its first appearance in the 1768

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

edition of the *Enquiry*. Prior to that, the passage read, "no Testimony for any kind of miracle *can ever possibly amount* to a Probability, much less a Proof" (emphasis added). This does seem like an a priori claim to the effect that it is impossible to obtain evidence sufficient for rational belief in the occurrence of a miracle. The 1768 wording Fogelin quotes does, I think, more accurately capture Hume's real intentions—a good empiricist like Hume would not really have intended to definitively rule out the possibility of any matter of fact (or its contrary). It would be antithetical to Hume's epistemology for him to claim that it would be literally impossible for there to be sufficient evidence that a miracle, conceived as a violation of a law of nature, had occurred. In any case, I will take Hume's final, most considered position as the relevant one for this analysis.

Comparing this passage to Fogelin (1990), we see that the intervening thirteen years have moderated Fogelin's position. In the earlier work Fogelin argues that Hume put forward an a priori argument intended to show that testimony, however strong, could never make it reasonable to believe that a miracle had occurred—a position Fogelin now considers "the first common misreading." On this point Fogelin now agrees with Flew (and with the position taken in this book). He has also given up the other claim he made in 1990: "Hume never presents [an a priori argument against the possibility of miracles], but it is tempting to think, as I once did, that it or something like it lies in the background, doing the primary argumentative work. I now think that this reading of the text is wholly mistaken" (Fogelin 2003, 17-8): "The error was largely due to a failure on my part to appreciate what Hume means by a proof" (Fogelin 2003, 91, n. 8).

Fogelin (2003) has it among his other aims to answer two recent critics of Hume on miracles, Johnson (1999) and Earman (2000). I will let what Fogelin says stand as a rebuttal of Johnson and turn instead to what Fogelin says about Earman. In the main, Fogelin comes to a diagnosis similar to my own: "What then are Earman's grounds for calling Hume's treatment of miracles a failure, indeed, an abject failure? As far as I can see, abuse aside, Earman's criticism turns on a single point. It depends fully on Earman's account of what Hume understands by an inductive argument amounting to a proof" (Fogelin 2003, 43).

The main problem is that Earman "attributes to Hume doctrines that are incompatible with central claims that Hume makes elsewhere in his writings. I am referring, of course, to his attribution of 'Hume's straight rule' to Hume" (Fogelin 2003, 55). If Hume were committed to the straight rule, Earman would have a point. But Hume is not so committed: "What is Earman's evidence in support of attributing Hume's straight rule to Hume? As far as I can see, it depends wholly on the occurrence of a number of strongly stated conclusions. He cites several examples of such strong talk" (Fogelin 2003, 45). (Above, I remarked on the texts that

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Earman explicitly cites to justify the attribution of the straight rule to Hume.) Fogelin argues that,

> although it is a mistake to cite Hume's strong talk as the basis for attributing to him a commitment to the straight rule, it would not be off the mark to say that sufficiently rich evidence could lead someone to speak and act *as if* or to speak and act *almost as if* a probability assignment of 1 (or 0) to a hypothesis is justified. When the evidence is strong enough to make something a moral certainty, then concern with further evidence ceases. (Fogelin 2003, 46)

Following a discussion of this point (a discussion with which I largely agree except for the attribution of numerical probabilities to Hume), Fogelin points out that looking at sections 11, 12, and 13 of Book 1, Part 3 of the *Treatise of Human Nature* (Of the Probability of Chances, Of the Probability of Causes, and Of Unphilosophical Probability) would have given Earman better information about Hume's theory of probability. This is certainly true. But then Fogelin writes, "If Earman had turned to [those sections], he would have encountered a position that, though mathematically naïve, is broadly Bayesian in character" (Fogelin 2003, 47). Fogelin cites Owen (1987) and Sobel (1987) as providing grounds for seeing Hume as an "intuitive" or "proto-" Bayesian. In my view, for reasons explained in chapters 4 and 5, this cannot be correct. Hume's view of evidential probability is not "mathematically naïve," it is entirely and deliberately *non*-mathematical.

One aspect of Fogelin's account appears to pose a significant challenge to the interpretation of Hume as having a non-Pascalian view of probability, although I think the challenge can be answered. Fogelin writes of Hume that,

> arguably his perhaps quaint account of the dispersion of a finite stock of vivacity (the source of belief) over available alternatives is presented in a way that yields the standard axioms of probability theory. More significantly, he seems to hold that "rational degrees of belief should be regimented according to the probability calculus." (Fogelin 2003, 47, quoting Earman 2000, 26)

In support of this claim, Fogelin cites Mura (1998). This turns out to be weak support indeed. Both Earman and Fogelin mention Mura's paper, but neither of them discuss its details. This may be because the details are extremely opaque. Mura's article is a mass of incorrect interpretations of Hume compounded with an unclear presentation of some quite technical ideas in mathematical probability theory. Mura suggests that we can read in Hume versions of several twentieth-century principles and rules of probability, and that these together entail the straight rule. I will not challenge Mura's claim that this cluster of rules entails the straight rule—although, if this is true, it is strange that Earman complains that the straight rule is incompatible with Bayesian principles. I will say, howev-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

er, that I think Mura is altogether wrong to attribute these rules to Hume. The textual evidence Mura cites is meager at best, and (as I have shown) there are independent textual and philosophical reasons to deny that Hume adopts the straight rule. Given that Mura's argument is wrong or at best extremely opaque, by appealing to Mura, Fogelin and Earman fail to establish that for Hume "rational degrees of belief should be regimented according to the probability calculus." The arguments gathered above for attributing to Hume a conception of probability that is incommensurable with the mathematical theory of probability thus stand unchallenged, despite appearances to the contrary.

There are admittedly many similarities between what Hume says about probability and what mathematical probability says; Mura is not totally off the mark. Making an inference from this similarity to the claim that Hume's theory of probability is identical with, or entails, or anticipates the mathematical theory of probability is, nevertheless, mistaken.

Thomas Kuhn's concept of the incommensurability of successive paradigms is applicable here. Just as Newtonian and Einsteinian physicists would understand most of each other's talk about "mass" but would ultimately mean something quite different by the term, the two paradigms of probability use similar language and, in some cases, seem to be saying very similar things although truly they are speaking different languages. Mura's inference is mistaken because attributing Bayesian or proto-Bayesian ideas to Hume inverts the order of dependence between the two. Bayesians, within the artificial framework of the mathematical calculus of probabilities (artificial in the sense that it does not fit very well with our natural reasoning about evidential probability), must try to come up with conclusions that are consistent with our ordinary (that is, non-mathematical) intuitions and considered conclusions about evidential probability. It is no surprise that Hume's view is paralleled in some respects by the conclusions of Bayesianism since the Bayesians (and Pascalians generally) must take conclusions like Hume's as the very phenomena to be accounted for within the mathematical framework. (Similarly, Hume certainly is not anticipating Tversky and Kahneman's [1982] mathematical treatment of "the errors that arise when partial beliefs are formed under the influence of exogenous or improperly weighted factors," as Fogelin [2003, 47–8] suggests.) Loose parallels of the sort Mura (1998) sees between Hume's statements on probability and Pascalian/Bayesian principles are therefore to be expected, but they certainly should not be interpreted as making Hume a proto-Bayesian. Hume's fundamental definitions regarding probability are incommensurable with those of the Pascalians.

To say that the tradition of non-mathematical probability anticipates Bayesianism is a bit like saying that Newton anticipates Einsteinian relativity theory. The two have some superficially similar terms, have some common aims, and in restricted cases their predictions coincide quite

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

closely. Really, though, the terms are not identical, and the predictions are not the same. What's more, the fundamental assumptions of the two theories make them incommensurable. Thus, while there are indeed similarities between Newtonian and Einsteinian discussions of gravity, focusing too much on the similarities disguises the much deeper and very significant differences between them. To take the analogy back to probability, mathematical and non-mathematical probability have some superficial similarities (because of their common origins and related interests), but their fundamental structures and principles are radically different. In fact, the differences are much more significant than the similarities. Hence it is wrong to claim that Hume's theory is "proto-Bayesian."

It is odd that Fogelin (2003) cites Mura (1998) approvingly, since one of Mura's main contentions is that Hume holds the straight rule of induction, something Fogelin correctly argues is a major interpretive error in Earman (2000). That Hume holds the straight rule is supposed to follow, according to Mura (1998), precisely from the fact that Hume is (as Mura thinks) implicitly discussing degrees of belief in terms of the probability calculus. It would seem, then, that Fogelin or anyone who wishes to attribute the probability calculus to Hume will be stuck with saying that Hume adopts the straight rule—unless, as I suspect, there is some other problem with Mura's argument.

My explanation of the loose parallels between some of Hume's statements about probability and the principles of the mathematical theory of probability—in addition to being true to the spirit and letter of Hume's writings—allows us to avoid attributing the straight rule to Hume. *Not* avoiding this would involve unnecessarily attributing to Hume an inconsistent position (as Fogelin 2003 mentions, there are explicit texts and arguments which show that Hume did not adopt the straight rule), which goes against good practice in history of philosophy.

To return to Fogelin's critique of Earman: After Fogelin discusses the fact that Earman incorrectly reads Hume's "strong talk" as implying the straight rule, he mentions some other problems with Earman's account. "More deeply, the attribution of such a straight rule to Hume seems to be flatly incompatible with one of Hume's most fundamental claims, namely that the 'course of nature may change'" (Fogelin 2003, 48). Fogelin is surely right about this; he cites THN *Abstract*, 14 and EHU 4.18 for textual support. Fogelin remarks that Hume's belief that the course of nature may change,

> seems to carry the immediate consequence (for him) that no inductive generalization (except one involving a complete enumeration) is unrevisable, is indefeasible, or has a conditional probability of 1. . . . [A]n uneliminable fallibilism lies at the heart of Hume's philosophy, and that on its face precludes attributing "Hume's straight rule" to him. (Fogelin 2003, 48)

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

From here Fogelin looks for direct textual evidence that Hume "speaks of something like a full proof's being open to revision" (Fogelin 2003, 49). This impulse is on the right track but, to my mind, he picks the wrong text. "Of Skepticism with Regard to Reason" (THN 1.4.1) makes a skeptical point about the foundations of deductive reasoning—a point that is comparable to the skeptical point Hume is more famous for having made with regard to the foundations of inductive reasoning. In a nutshell, Hume's argument is that we cannot demonstrate the certainty of the conclusions of deductive reasoning because doing so would require an infinite regress of checks of our reasoning, and that in the end we trust the results of deductive reasoning because of a natural tendency of the mind to discount the checks beyond the first step or two. "Hume is not questioning the status of the demonstrative sciences themselves, but merely calling attention to our inherent fallibility as practitioners of these sciences and endeavoring to make us face up to the implications of this fallibility" (Allison 2008, 212).

An analogous point applies to probable reasoning. Thus custom and (arational) human cognitive psychology play as central a role in deductive reasoning as they do in inductive reasoning, and hence there is no way to guarantee the truth (or probability) of conclusions of deductive (or inductive) reasoning. Fogelin uses Hume's "Of Skepticism with Regard to Reason" to show that Hume would be willing to revise downward the degree of certainty attached to something close to full proof: Hume argues (THN 1.4.1.6) that at each step of checking the probability of a probable inference, new doubts are introduced until finally "there remain[s] nothing of the original probability, however great we may suppose it to have been." That is, even if we start with something that would count as "full proof," we end up judging that it has *no probability*.

All this is true of Hume, of course, but it does not support the conclusion Fogelin seeks to establish. Fogelin's argument in this instance mixes together levels of analysis that Hume explicitly separates. In the very next section of the *Treatise*, after all, Hume proposes "A Skeptical Solution of These Doubts": The skeptical conclusions regarding the foundations of deductive and inductive reasoning are not meant to stand in full force after that point.

In his solution to the skeptical doubts regarding demonstrative reason, Hume distinguishes our philosophical moods from ordinary life. He remarks on several occasions that the skeptical doubts we convince ourselves of in the dark of our rooms unavoidably vanish when we step out into the light of day and go about our ordinary lives (for example, THN 1.4.2.57). We may have doubts about demonstrative or probable reasoning while in our philosophical moods, but in ordinary life we use demonstrative and probable reasoning without difficulty and without skeptical worry. Moreover, it is unavoidable *and perfectly rational* to do so. Hume's view, in the final analysis, is that there *are* normative standards for the

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

probable reasoning employed in ordinary life. Despite skeptical doubts about the foundations of induction, at a non-foundational level there are definitely right and definitely wrong ways to reason about probabilities. Similarly, ordinary standards of deductive validity and soundness apply to demonstrative reasoning in ordinary life, even if we can find reasons to be skeptical of those standards during our philosophical moods. Establishing this is the point of the section "Of Unphilosophical Probability" (THN 1.3.13).

I discussed in chapter 6 how Hume can get away with this apparently contradictory skeptical/normative move. For present purposes, grant that Hume's epistemic normativism is licensed, and let me point out why, as a result, Fogelin cannot use Hume's remarks in "Of Skepticism with Regard to Reason" as grounds for thinking that Hume does not adopt the straight rule. Fogelin concludes his brief discussion of "Of Skepticism with Regard to Reason" with the claim that the skeptical argument of THN 1.4.1.6 "is not an argument that someone committed to the straight rule would give" (Fogelin 2003, 50). But this is not the case. Given my remarks in the previous paragraph, we can see that Hume could perfectly well grant the skeptical point that there is no rational foundation for probable inferences, or that really they have no probability, and yet with regard to ordinary life (in what we might call "scientific" rather than "philosophical" moments) normatively insist that good probable inferences must be governed by the straight rule (or by some other inductive inference rule). I hold, with Fogelin, that the straight rule is in fact not Hume's inductive rule (in ordinary life or anywhere); but I also think, against Fogelin, that the evidence to establish this must be found in a different place.

Worse for Fogelin, if the doubts in "Of Skepticism with Regard to Reason" were to be used in the way he suggests, the whole of Hume's argument against miracles would be undercut. Hume would be forced to say that the "proofs" of the laws of nature have no epistemic weight. The proofs from testimony about violations of the laws of nature would in this case also come to nothing, of course, but Hume is not rejecting belief in miracles on the basis of a radical skepticism according to which there are no warranted beliefs at all. He is in fact so far from being radically skeptical in "Of Miracles" as to be nearly rabidly epistemically normative. Hume clearly thinks it is incorrect to believe in miracles, *because* the overall evidence is so strongly against them. Belief in a religious hypothesis on the basis of reports of miracles is thus foolish, not wise. Hume would not be able to draw strong conclusions along these lines if he were really being radically skeptical in "Of Miracles" in the way that Fogelin's argument invoking "Of Skepticism with Regard to Reason" would require. In any case, Hume's claim that even the best supported empirical generalization is subject to revision (for various reasons, including the fact that "the course of nature may change") is made at the level of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

ordinary epistemology, not the level of foundations (where the skeptical points are to be found).

Fogelin's second text to support the contention that Hume thinks of even full proofs as open to revision, and thus to show that the straight rule does not apply to Hume, is "Of Miracles" itself (Fogelin 2003, 50). I generally concur. The straight rule does not fit what Hume says there (Earman shows this: for example, Earman 2000, 32). "Of Miracles" is consistent with, and built upon, Hume's epistemology as developed elsewhere in the *Enquiry* and the *Treatise* (as I have shown in this book); it follows that the straight rule is not Hume's inductive rule.

Fogelin's overall conclusion begins as follows: "With respect to miracles, Hume's strategy is to use the canons of causal reasoning to evaluate testimony brought forward in their behalf. Because, for him, no matter of fact can be established a priori, it remains an open, though remote possibility that testimony could establish the occurrence of a miracle. That is the point of the discussion of the eight days of darkness" (Fogelin 2003, 62).

So far, so good. But given Fogelin's care in other parts of his book to stick close to Hume's actual arguments, and his criticisms of others for putting words in Hume's mouth, the continuation of this passage comes as a surprise: "With respect to miracles intended to serve as a foundation of a religion, the situation, according to Hume, is *factually* different. When we examine the testimony brought forward to in their behalf, we see that it *uniformly* failed to meet appropriate standards of acceptability. This uniform unreliability provides us with a proof—not a demonstration—that testimony offered in behalf of religious miracles cannot be trusted" (Fogelin 2003, 62).

As far as I can see, Hume never draws any inference of the form "there is a uniformity of bad testimony to religious miracles, therefore all testimony to religious miracles is untrustworthy." Hume says things *around* these points, but never makes the argument Fogelin here attributes to him.

There are three reasons to doubt that Hume makes such an argument. The first is that Hume is willing to grant that testimony *could* be sufficient to count as a proof of a miracle, as in the example of the eight days of darkness.

The second reason to doubt that Hume makes Fogelin's "uniformly bad testimony to miracles" argument is that he does not need it. Hume draws a general conclusion about the balance of probabilities from competing evidence regarding laws of nature and their supposed violations yielding (even in the best case for miracle-claimants) almost no credibility. The evidence against the credibility of miracle reports is normally very strong, and even in the most miracle-friendly situation the evidence for the occurrence of the miracle will still be very weak. For this reason

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

alone, testimony about miracles cannot be a foundation for a religious hypothesis.

The third reason to doubt that Hume makes Fogelin's "uniformly bad testimony to miracles" argument is that it begs the question, or comes dangerously close to doing so at least. What Hume is investigating is the credibility of miracle reports. He should not assume that all miracle reports are unreliable, and he certainly cannot claim to have examined *every* miracle report in detail. Hume's argument is in fact much subtler. We know of *some* instances where miracle reports have been fraudulent or delusional. We suspect others as well, but without direct evidence. We know that testimony in general is sometimes (although not usually) fallible, and that testimony to unusual events tends to be more unreliable than normal. We notice in addition that miracle reports usually arise among people who are not well educated and who tend to be superstitious and overly credulous. Being uneducated and superstitious does not automatically make someone's testimony unreliable, but it does give others grounds for doubting the reliability of his or her testimony about miracles. Compare this against the uniform, highly reliable evidence for the laws of nature, with respect to which we have no grounds for doubt.

Hume's argument is thus not that we have grounds to doubt *each* miracle report, but that the bad reports we do know about cast doubt on the others. Hume's is a *type*-level rather than a *token*-level analysis. And moreover (Hume establishes), *even if* we had excellent reasons to trust a given miracle report, we would still have to weigh that report against the extremely strong evidence for the laws of nature. Given that this subtler argument can be extracted from Hume's text, we are able to avoid attributing to him a circular argument. Whether or not individual miracle reports are grounds for doubting the uniformity of laws is precisely what is in question, so Hume correctly leaves them out of the premises of his argument.

Effectively, Fogelin's strategy against Earman is to argue, first, that Earman gives the wrong Bayesian analysis and, second, that better analyses (for example, Owen 1987 or Dawid and Gillies 1989) show that Hume does not fall into the errors Earman attributes to him. My view, in contrast, is that Earman goes wrong—as do all Bayesian analyses, including Fogelin's—by attributing to Hume the schema of mathematical probability. Fogelin's defense of Hume has the right intuitive thrust, but because the history of ideas has forgotten the non-mathematical theory of probability he makes interpretive errors that undermine the details of his account.

Otte (2005) points out that there is a structural problem in Fogelin's rebuttal of Earman, namely that "all of Fogelin's criticisms of Earman are based on Earman's attribution of the straight rule to Hume. . . . But although it is true that Earman thinks that Hume adopts the straight rule, much (if not most) of the discussion is not based on this claim. . . . [I]n

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

later chapters of his book he explicitly assumes the probability of a miracle is greater than 0" (Otte 2005, 167). This means Fogelin does not discuss Earman's treatment of Hume's "general maxim" or main thesis. My account in chapter 5 does consider these points and thus improves upon Fogelin's defense of Hume from Earman's attack.

CONCLUSION OF CHAPTER 7

I have argued here that, while Fogelin's instincts and most of his claims about the argument against miracles are correct, in some important details he misreads Hume, and misattributes certain views to him. Effectively, Fogelin's strategy against Earman is to argue, first, that Earman gives the wrong Bayesian analysis and, second, that better analyses show that Hume does not fall into the errors Earman attributes to him. My view, in contrast, is that Earman goes wrong—as do all Bayesian analyses, including Fogelin's—by attributing to Hume the scheme of mathematical probability. Besides a misguided reliance on Mura (1998), Fogelin uses the wrong texts to support his position that the straight rule is not Hume's inductive rule; if Fogelin were correct about his explanation of why Hume does not accept the straight rule, that explanation would undercut Hume's argument against miracles as well. However, a better reading yields a subtle and powerful argument against belief in miracles that is fully integrated with Hume's empiricism.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Epilogue

GENERAL IMPLICATIONS FOR HISTORIANS OF IDEAS AND PHILOSOPHERS OF SCIENCE

Recovering the lost memory of the non-mathematical theory of probability that was dominant before the rise of the mathematical theory is good not just because it allows scholars to better understand Hume, an important figure in the history of ideas. It also provides a clearer lens through which historians of philosophy and historians of science can view many of their subjects. With this in our toolkit, we are less likely to radically misinterpret the positions of philosophers and scientists from the past. It may also provide conceptual resources for contemporary philosophy of science, and science itself, to analyze current issues and problems in a less overblown and presumptuous way.

Let me be clear that I do agree that there are a great many correct applications of the mathematical theory of probability in life and in science. I am not suggesting a general replacement of all mathematical accounts of probability with non-mathematical accounts. There are clearly contexts in which mathematical probability provides good and true answers. These range from analyses of simple games of chances, to comparative studies of the relative effectiveness of medical treatments, to actuarial estimates of insurance risks.

There are, however, clearly many contexts in which the mathematical theory of probability is at least unhelpful, can be potentially misleading, and is even positively harmful. There are also cases where ordinary reasoning about probability is perfectly adequate and attempts to promote mathematical precision do not add anything of value. The Dutch Book argument may provide "conditions of rationality" in some kinds of evidential contexts, for example when the logical space of contrary propositions is fully known, and statistical reasoning applies, but as I have argued this does not mean that every evidential situation can or should be described in mathematical terms.

The allure of rigor and precision is one reason for the dominance of mathematical probability. We aspire to be scientific, and thus to be able to give precise numerical accounts in science, life, and policy. Too often, though, this amounts to scientism, and the appearance of precision is nothing but an illusion.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Sometimes (as, for example, in many cases of comparing causal hypotheses in science) mathematical probability is inapplicable or unhelpful simply because we do not know all the possible alternatives and therefore cannot apply the principle of equiprobability to estimate the chances of something occurring against the background of possibilities. Sometimes (as, for example, in some cases of trying to disentangle medical etiologies, or in attempts to project the consequences of global climate change) mathematical probability is inapplicable or unhelpful because the causal interactions are too complex for us to know or calculate, and our attempts to quantify probabilities end up being weakly founded guesses built on assumptions or arbitrary conventions, rather than being the rigorously precise measurements they purport to be. Other times, I contend, we try to apply the mathematical theory of probability to problem situations to which it simply does not apply. Insisting upon a mathematical analysis in these three types of cases often leads to misleading results. In some of these cases, the non-mathematical account of probability may turn out to be a useful and appropriate tool. When that is so, we are able to make reasoned, warranted judgments about absolute and/or relative probabilities that are sufficient for belief and action even though they are not expressed in a mathematical framework.

Barry Gower (1987) illustrates that there certainly has not been uniform agreement about the application of the probability calculus to scientific problems in the history of science. He describes the example Bernoulli's and d'Alembert's debate on the question of assessing the probability of the occurrence that the inclinations of the planetary orbits in our solar system would turn out to be as close to co-planar as they are (within just a few degrees, not counting Neptune):

> In d'Alembert's view, the concepts employed in the probability calculus needed careful and critical scrutiny so as to ensure that applications of the calculus would be reliable. In particular, given the prominence accorded to judgements of equiprobability and equipossibility, it was essential that the nature of these probabilities and possibilities be clarified. We need, said d'Alembert, to distinguish an abstract and mathematical use of the concepts from a concrete and physical use. It is one thing to exercise our powers of reasoning to identify and quantify real objective possibilities; it is quite another to ground such possibilities in experience. (Gower 1987, 448)

Even Hacking has argued that there are some applications of mathematical probability that produce results that are simply beyond the pale. Speaking of the "epistemic use of probability to infer conclusions on the basis of available evidence" (1989, 414), Hacking mentions research on cosmology and on purported psychic phenomena as two areas in which "astronomical" probabilities—billions or trillions to one—are regularly

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

cited as supposed evidence for conclusions, and which he says we should be very happy to ignore.

We could, I think, place in the same category Richard Price's (1767) attempt to refute Hume's thesis in "Of Miracles" by proposing to show that contrary to Hume's claim, it is not the case that uniform regularities of past experience make it so that we can "never" expect the regularity to be broken. Price used Bayes theorem to calculate, for example, that after one million observations of the tide coming in, there is a 50 percent chance that the probability of the tide not coming in one day is between 1 in 600,000 and 1 in 3,000,000. This is supposed to show that we do not have an expectation of a perfect similarity of future to past experience even after extremely extensive experience of regularity. Hume, I think, would point out that he wasn't trying to argue that miracles (as violations of regularities of experience) are impossible, so Price has somewhat missed the point. Hume would also say that if there is only a 50 percent chance that the tide will fail to come in 1 in 3 million times, we should not accept suspect testimonial evidence that it had in fact failed to come in—since the event is so completely unlikely, it is much more likely (given what we know about human testimony) that the report is wrong. Hacking might say that extreme probabilities like these are essentially meaningless and should be ignored.

Instead of here reviewing examples of scientific misfires in attempts to mathematically "probabilify" hypotheses given empirical evidence, let it suffice to say that there are many examples of such misfires. Worse, the most fundamental problem in the logic of evidence, the problem of the confirmation of scientific theories by the available evidence, has hardly been helped by a move to a mathematical framework. If anything, mathematizing epistemic probabilities has in most cases served only to stultify progress. Bayesianism does not solve the problem of induction; it just sidesteps or ignores that problem.

The failure to solve the problems in the logic of evidence and confirmation is a significant factor in the demise of the Logical Empiricist framework and the rise of irrationalist philosophies of science from Thomas Kuhn to Paul Feyerabend to Bruno Latour. The turn to the sociology and the history of science (including, for example, material culture, institutional history, and participant-observer studies) has indeed led to valuable contributions to our understanding of science and the history of ideas—though, often, at the expense of the *ideas*.

Perhaps the right view is the pluralism about probability advocated in chapter 5. We could do worse than to recognize that there are several different kinds of probability that apply in different kinds of contexts, and to then more thoughtfully consider how to use probable reasoning in specific contexts. In some contexts, a non-mathematical approach to probability such as Hume's might be best.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

SPECIFIC IMPLICATIONS FOR THE STUDY OF HUME

With the general background on probability and its history in mind, my goal in this book has been to show that, taken on its own terms and understood within the context of his overall thought and the history of evidential reasoning, Hume's position on the incredibility of miracles is plausible. In a nutshell, Hume's position on miracles is that, because of the way humans come to know empirical matters of fact, the warrant for a report of a violation of an otherwise-exceptionless regularity of experience (a law of nature) is so unlikely to rise to the level required for reasonable belief in the occurrence of the supposed singular event that we can safely say that it will never in fact rise to that level. The evidence for a law of nature, since exceptionless, provides us with a *proof* of the law, which is to say that there is no room for reasonable doubt and it would be ridiculous to deny the law given the available evidence. Testimonial evidence, though normally reliable, is also nevertheless fallible, and it is especially suspect in cases of reports of unusual events. Thus, a report of a miracle will always, in fact (though *not* in principle), have a much lower degree of probability than the law the miracle supposedly violates. This is itself a probable statement. Someone who accepts Hume's line of reasoning will agree that it has a degree of probability attaching to it that makes it *certain for all practical purposes*.

Besides correcting some common misreadings of Hume's position, I have shown that while Hume's remarks on probability sometimes seem strange in our era, this is mostly because we have lost sight of the ancient tradition of probability in which Hume participates. Though entirely non-mathematical, that tradition is plausible as an account of epistemic probability—not least because numerical or Pascalian probability, of which Bayesianism is an example, has not shown itself to be able to improve upon the conclusions of the non-numerical tradition in many spheres of analysis. While Coleman and Cohen have already argued that Hume's theory of probability is non-mathematical in this way, my work is the first to explicitly draw the direct connection between Hume's theory of probability and the long and laudable tradition of reasoning about evidential probability that goes back to ancient Roman and medieval law.

In the prologue, I said that there were five main aims of this book. Having given a clear statement of the assumptions, structure, and conclusions of Hume's argument against believing in miracles, and having carefully put it into historical and philosophical context, we can see that Hume's argument is plausible in its own right. I have provided what I take to be a definitive defense of Hume from Earman's attack, in part by correcting those instances in which Fogelin's answers to Earman go awry and in part by revealing additional errors in Earman's account. I developed and contextualized Coleman's analysis of Hume as a non-Pascalian

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

about probability. Then, in part through making clear that Hume is a participant in a neglected tradition of probability that existed for hundreds of years before Pascal developed the mathematical theory of probability, I connected Hume to a tradition of probable reasoning that goes back to medieval and Roman sources. I have suggested, too, that this older tradition of probability could even today function productively as a framework for understanding evidence.

I have no practical doubt that the conversation on Hume on miracles will continue. There is too much at stake for either side in the debate to rest satisfied that their work is done, and the case sufficiently communicated, despite Hume's state hope that, at least for the wise and learned, his case against believing in miracles would be an everlasting check on superstition and delusion. There is, moreover, such a host of interesting issues to consider that many people will be inspired to join the conversation. I do hope, though, that commentators will now stop trying to give Bayesian or other mathematical analyses of Hume on probability, which cannot possibly correctly represent Hume's own view.

It seems fitting to give Hume the final word:

> Upon the whole, then, it appears that no testimony for any kind of miracle has ever amounted to a probability, much less to a proof; and that, even supposing it amounted to a proof, it would be opposed by another proof; derived from the very nature of the fact, which it would endeavour to establish [namely, a violation of a law of nature]. It is experience only, which gives authority to human testimony; and it is the same experience, which assures us of the laws of nature. When, therefore, these two kinds of experience are contrary, we have nothing to do but subtract the one from the other, and embrace an opinion, either on one side or the other, with that assurance which arises from the remainder. But according to the principle here explained, this subtraction, with regard to all popular religions, amounts to an entire annihilation; and therefore we may establish it as a maxim, that no human testimony can have such force as to prove a miracle, and make it a just foundation for any such system of religion. (EHU 10.35)

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

A Brief Biography of Hume

David Hume (1711–1776) is one of the most influential philosophers ever to have written in the English language. His comments on many topics in epistemology and metaphysics remain the standard starting place for philosophical discussions even today. His views about causation, induction, and philosophy of religion are usually considered to be his most important contributions. His most significant works were *A Treatise of Human Nature*, first published in 1739 when Hume was just twenty-eight years old, and a shorter "popularization" of the epistemological parts of that work, *An Enquiry concerning Human Understanding*, first published in 1748, with a third edition appearing posthumously in 1777. Hume also published on moral and political philosophy, economics, and other topics. In his own lifetime he was best known and most admired for his *History of England* (first published in six volumes, 1754–1762).

Hume was born, lived much of his life, and died in Edinburgh, the political and intellectual capital of Scotland. His father died when he was about two years old, and his mother never remarried. Hume had the run of his father's extensive library at home, which may have been one of the sparks that led to his lifelong love of the life of the mind. Hume attended the University of Edinburgh with his older brother. After ceasing his undergraduate studies—which exposed him to classical languages and literature, and provided him with meaningful exposure to the philosophy, mathematics, and natural science of his day as well as a background in ancient philosophy—Hume then enrolled as a law student at Edinburgh for three years.

Hume was a major part of what is now termed "The Scottish Enlightenment" (roughly 1740–1790), a period of intellectual flourishing and innovation of remarkable breadth and depth. Other key figures in the period include Thomas Reid, Adam Smith, Francis Hutcheson, Lord Kames, and others, many of whom were Hume's personal friends and acquaintances. (Hume goes against the stereotype of a philosopher in that he was very skilled at being social.) At various points in his life Hume served as tutor to a young nobleman, librarian to a society of lawyers in Edinburgh, aide-de-camp on a military expedition, and diplomat. He sought but was denied a professorship in philosophy at Edinburgh—the conservative Presbyterian establishment objected to Hume's candidacy because they considered him to be a skeptic and an atheist. In the end Hume attained lasting international fame, and a modest fortune,

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

by being what he had from youth desired to be: "a man of letters." His many publications in philosophy, politics, and history reached a wide audience in his own day, and most remain in print today. Hume achieved both literary fame and wealth from his writing. He was greatly admired by his friends, even those with whom he disagreed philosophically and religiously, for his generous, engaged, curious, and sociable temperament. He died in 1777, probably from stomach cancer, and maintained his cheerful, open disposition until the end.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Hume's "Of Miracles"

Chapter 10 of
An Enquiry concerning Human Understanding

Below is reprinted the main text of the chapter "Of Miracles." The text is taken from the posthumous 1777 edition of EHU, which Hume himself prepared. (I have included only two of Hume's notes, the ones which bear most importantly on the main argument. The rest are of minor or historical significance and they can be found in EHU for those interested.)

PART I.

1. There is, in DR. TILLOTSON's writings, an argument against the *real presence*, which is as concise, and elegant, and strong as any argument can possibly be supposed against a doctrine, so little worthy of a serious refutation. 'Tis acknowledged on all hands, says that learned prelate, that the authority, either of the scripture or of tradition, is founded merely in the testimony of the apostles, who were eye-witnesses to those miracles of our Saviour, by which he proved his divine mission. Our evidence, then, for the truth of the *Christian* religion is less than the evidence for the truth of our senses; because, even in the first authors of our religion, it was no greater; and 'tis evident it must diminish in passing from them to their disciples; nor can any one rest such confidence in their testimony, as in the immediate object of his senses. But a weaker evidence can never destroy a stronger; and therefore, were the doctrine of the real presence ever so clearly revealed in scripture, it were directly contrary to the rules of just reasoning to give our assent to it. It contradicts sense, tho' both the scripture and tradition, on which it is supposed to be built, carry not such evidence with them as sense; when they are considered merely as external evidences, and are not brought home to every one's breast, by the immediate operation of the Holy Spirit.

2. Nothing is so convenient as a decisive argument of this kind, which must at least *silence* the most arrogant bigotry and superstition, and free us from their impertinent solicitations. I flatter myself, that I have discovered an argument of a like nature, which, if just, will, with the wise and learned, be an everlasting check to all kinds of superstitious delusion, and consequently, will be useful as long as the world endures. For so

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

long, I presume, will the accounts of miracles and prodigies be found in all history, sacred and profane.

3. Tho' experience be our only guide in reasoning concerning matters of fact; it must be acknowledged, that this guide is not altogether infallible, but in some cases is apt to lead us into errors. One, who in our climate, should expect better weather in any week of JUNE than in one of DECEMBER, would reason justly, and conformably to experience; but 'tis certain, that he may happen, in the event, to find himself mistaken. However, we may observe, that, in such a case, he would have no cause to complain of experience; because it commonly informs us beforehand of the uncertainty, by that contrariety of events, which we may learn from a diligent observation. All effects follow not with like certainty from their supposed causes. Some events are found, in all countries and all ages, to have been constantly conjoined together: Others are found to have been more variable, and sometimes to disappoint our expectations; so that, in our reasonings concerning matter of fact, there are all imaginable degrees of assurance, from the highest certainty to the lowest species of moral evidence.

4. A wise man, therefore, proportions his belief to the evidence. In such conclusions as are founded on an infallible experience, he expects the event with the last degree of assurance, and regards his past experience as a full *proof* of the future existence of that event. In other cases, he proceeds with more caution: He weighs the opposite experiments: He considers which side is supported by the greater number of experiments: To that side he inclines, with doubt and hesitation; and when at last he fixes his judgment, the evidence exceeds not what we properly call *probability*. All probability, then, supposes an opposition of experiments and observations; where the one side is found to overbalance the other, and to produce a degree of evidence, proportioned to the superiority. A hundred instances or experiments on one side, and fifty on another, afford a doubtful expectation of any event; tho' a hundred uniform experiments, with only one that is contradictory, reasonably beget a pretty strong degree of assurance. In all cases, we must balance the opposite experiments, where they are opposite, and deduct the smaller number from the greater, in order to know the exact force of the superior evidence.

5. To apply these principles to a particular instance; we may observe, that there is no species of reasoning more common, more useful, and even necessary to human life, than that which is derived from the testimony of men, and the reports of eye-witnesses and spectators. This species of reasoning, perhaps, one may deny to be founded on the relation of cause and effect. I shall not dispute about a word. It will be sufficient to observe, that our assurance in any argument of this kind is derived from no other principle than our observation of the veracity of human testimony, and of the usual conformity of facts to the reports of witnesses. It

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

being a general maxim, that no objects have any discoverable connexion together, and that all the inferences, which we can draw from one to another, are founded merely on our experience of their constant and regular conjunction; 'tis evident, that we ought not to make an exception to this maxim in favour of human testimony, whose connexion with any event seems, in itself, as little necessary as any other. Were not the memory tenacious to a certain degree; had not men commonly an inclination to truth and a principle of probity; were they not sensible to shame, when detected in a falsehood: Were not these, I say, discovered by *experience* to be qualities, inherent in human nature, we should never repose the least confidence in human testimony. A man delirious, or noted for falsehood and villany, has no manner of authority with us.

6. And as the evidence, derived from witnesses and human testimony, is founded on past experience, so it varies with the experience, and is regarded either as a *proof* or a *probability*, according as the conjunction between any particular kind of report and any kind of object has been found to be constant or variable. There are a number of circumstances to be taken into consideration in all judgments of this kind; and the ultimate standard, by which we determine all disputes, that may arise concerning them, is always derived from experience and observation. Where this experience is not entirely uniform on any side, 'tis attended with an unavoidable contrariety in our judgments, and with the same opposition and mutual destruction of argument as in every other kind of evidence. We frequently hesitate concerning the reports of others. We balance the opposite circumstances, which cause any doubt or uncertainty; and when we discover a superiority on any side, we incline to it; but still with a diminution of assurance, in proportion to the force of its antagonist.

7. This contrariety of evidence, in the present case, may be derived from several different causes; from the opposition of contrary testimony; from the character or number of the witnesses; from the manner of their delivering their testimony; or from the union of all these circumstances. We entertain a suspicion concerning any matter of fact, when the witnesses contradict each other; when they are but few, or of a doubtful character; when they have an interest in what they affirm; when they deliver their testimony with hesitation, or on the contrary, with too violent asseverations. There are many other particulars of the same kind, which may diminish or destroy the force of any argument, derived from human testimony.

8. Suppose, for instance, that the fact, which the testimony endeavours to establish, partakes of the extraordinary and the marvellous; in that case, the evidence, resulting from the testimony, admits of a diminution, greater or less, in proportion as the fact is more or less unusual. The reason, why we place any credit in witnesses and historians, is not derived from any *connexion*, which we perceive *à priori*, between testimony and reality, but because we are accustomed to find a conformity between

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

them. But when the fact attested is such a one as has seldom fallen under our observation, here is a contest of two opposite experiences; of which the one destroys the other, as far as its force goes, and the superior can only operate on the mind by the force, which remains. The very same principle of experience, which gives us a certain degree of assurance in the testimony of witnesses, gives us also, in this case, another degree of assurance against the fact, which they endeavour to establish; from which contradiction there necessarily arises a counterpoize, and mutual destruction of belief and authority.

9. *I should not believe such a story were it told me by* CATO; was a proverbial saying in ROME, even during the lifetime of that philosophical patriot. The incredibility of a fact, it was allowed, might invalidate so great an authority.

10. The INDIAN prince, who refused to believe the first relations concerning the effects of frost, reasoned justly; and it naturally required very strong testimony to engage his assent to facts, that arose from a state of nature, with which he was unacquainted, and which bore so little analogy to those events, of which he had had constant and uniform experience. Though they were not contrary to his experience, they were not conformable to it.[1]

11. But in order to encrease the probability against the testimony of witnesses, let us suppose, that the fact, which they affirm, instead of being only marvellous, is really miraculous; and suppose also, that the testimony, considered apart and in itself, amounts to an entire proof; in that case, there is proof against proof, of which the strongest must prevail, but still with a diminution of its force, in proportion to that of its antagonist.

12. A miracle is a violation of the laws of nature; and as a firm and unalterable experience has established these laws, the proof against a miracle, from the very nature of the fact, is as entire as any argument from experience can possibly be imagined. Why is it more than probable, that all men must die; that lead cannot, of itself, remain suspended in the air; that fire consumes wood, and is extinguished by water; unless it be, that these events are found agreeable to the laws of nature, and there is required a violation of these laws, or in other words, a miracle to prevent them? Nothing is esteemed a miracle, if it ever happen in the common course of nature. 'Tis no miracle that a man, seemingly in good health, should die on a sudden; because such a kind of death, tho' more unusual than any other, has yet been frequently observed to happen. But 'tis a miracle, that a dead man should come to life; because that has never been observed, in any age or country. There must, therefore, be an uniform experience against every miraculous event, otherwise the event would not merit that appellation. And as an uniform experience amounts to a proof, there is here a direct and full *proof*, from the nature of the fact, against the existence of any miracle; nor can such a proof be destroyed, or

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

the miracle rendered credible, but by an opposite proof, which is superior.[2]

13. The plain consequence is (and 'tis a general maxim worthy of our attention), "That no testimony is sufficient to establish a miracle, unless the testimony be of such a kind, that its falsehood would be more miraculous, than the fact, which it endeavours to establish: And even in that case, there is a mutual destruction of arguments, and the superior only gives us an assurance suitable to that degree of force, which remains, after deducting the inferior." When any one tells me, that he saw a dead man restored to life, I immediately consider with myself, whether it be more probable, that this person should either deceive or be deceived, or that the fact, which he relates, should really have happened. I weigh the one miracle against the other; and according to the superiority, which I discover, I pronounce my decision, and always reject the greater miracle. If the falsehood of his testimony would be more miraculous, than the event which he relates; then, and not till then, can he pretend to command my belief or opinion.

PART II.

14. In the foregoing reasoning we have supposed, that the testimony, upon which a miracle is founded, may possibly amount to an entire proof, and that the falsehood of that testimony would be a real prodigy: But 'tis easy to shew, that we have been a great deal too liberal in our concession, and that there never was a miraculous event established on so full an evidence.

15. For *first*, there is not to be found, in all history, any miracle attested by a sufficient number of men, of such unquestioned good-sense, education, and learning, as to secure us against all delusion in themselves; of such undoubted integrity, as to place them beyond all suspicion of any design to deceive others; of such credit and reputation in the eyes of mankind, as to have a great deal to lose in case of their being detected in any falsehood; and at the same time, attesting facts, performed in such a public manner, and in so celebrated a part of the world, as to render the detection unavoidable: All which circumstances are requisite to give us a full assurance in the testimony of men.

16. *Secondly*. We may observe in human nature a principle, which, if strictly examined, will be found to diminish extremely the assurance, which we might, from human testimony, have, in any kind of prodigy. The maxim, by which we commonly conduct ourselves in our reasonings, is, that the objects, of which we have no experience, resemble those, of which we have; that what we have found to be most usual is always most probable; and that where there is an opposition of arguments, we ought to give the preference to such as are founded on the greatest number of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

past observations. But tho', in proceeding by this rule, we readily reject any fact which is unusual and incredible in an ordinary degree; yet in advancing farther, the mind observes not always the same rule; but when any thing is affirmed utterly absurd and miraculous, it rather the more readily admits of such a fact, upon account of that very circumstance, which ought to destroy all its authority. The passion of *surprize* and *wonder*, arising from miracles, being an agreeable emotion, gives a sensible tendency towards the belief of those events, from which it is derived. And this goes so far, that even those who cannot enjoy this pleasure immediately, nor can believe those miraculous events, of which they are informed, yet love to partake of the satisfaction at second-hand or by rebound, and place a pride and delight in exciting the admiration of others.

17. With what greediness are the miraculous accounts of travellers received, their descriptions of sea and land monsters, their relations of wonderful adventures, strange men, and uncouth manners? But if the spirit of religion join itself to the love of wonder, there is an end of common sense; and human testimony, in these circumstances, loses all pretensions to authority. A religionist may be an enthusiast, and imagine he sees what has no reality: He may know his narrative to be false, and yet persevere in it, with the best intentions in the world, for the sake of promoting so holy a cause: Or even where this delusion has not place, vanity, excited by so strong a temptation, operates on him more powerfully than on the rest of mankind in any other circumstances; and self-interest with equal force. His auditors may not have, and commonly have not, sufficient judgment to canvass his evidence: What judgment they have, they renounce by principle, in these sublime and mysterious subjects: Or if they were ever so willing to employ it, passion and a heated imagination disturb the regularity of its operations. Their credulity encreases his impudence: And his impudence overpowers their credulity.

18. Eloquence, when at its highest pitch, leaves little room for reason or reflection; but addressing itself entirely to the fancy or the affections, captivates the willing hearers, and subdues their understanding. Happily, this pitch it seldom attains. But what a TULLY or a DEMOSTHENES could scarcely effect over a Roman or Athenian audience, every *Capuchin*, every itinerant or stationary teacher can perform over the generality of mankind, and in a higher degree, by touching such gross and vulgar passions.

19. The many instances of forged miracles, and prophecies, and supernatural events, which, in all ages, have either been detected by contrary evidence, or which detect themselves by their absurdity, prove sufficiently the strong propensity of mankind to the extraordinary and the marvellous, and ought reasonably to beget a suspicion against all relations of this kind. This is our natural way of thinking, even with regard to the most common and most credible events. For instance: There is no kind of

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

report, which rises so easily, and spreads so quickly, especially in country places and provincial towns, as those concerning marriages; insomuch that two young persons of equal condition never see each other twice, but the whole neighbourhood immediately join them together. The pleasure of telling a piece of news so interesting, of propagating it, and of being the first reporters of it, spreads the intelligence. And this is so well known, that no man of sense gives attention to these reports, till he find them confirmed by some greater evidence. Do not the same passions, and others still stronger, incline the generality of mankind to believe and report, with the greatest vehemence and assurance, all religious miracles?

20. *Thirdly*. It forms a strong presumption against all supernatural and miraculous relations, that they are observed chiefly to abound among ignorant and barbarous nations; or if a civilized people has ever given admission to any of them, that people will be found to have received them from ignorant and barbarous ancestors, who transmitted them with that inviolable sanction and authority, which always attend received opinions. When we peruse the first histories of all nations, we are apt to imagine ourselves transported into some new world; where the whole frame of nature is disjointed, and every element performs its operations in a different manner, from what it does at present. Battles, revolutions, pestilence, famine, and death, are never the effect of those natural causes, which we experience. Prodigies, omens, oracles, judgments, quite obscure the few natural events, that are intermingled with them. But as the former grow thinner every page, in proportion as we advance nearer the enlightened ages, we soon learn, that there is nothing mysterious or supernatural in the case, but that all proceeds from the usual propensity of mankind towards the marvellous, and that, tho' this inclination may at intervals receive a check from sense and learning, it can never be thoroughly extirpated from human nature.

21. *'Tis strange*, a judicious reader is apt to say, upon the perusal of these wonderful historians, *that such prodigious events never happen in our days*. But 'tis nothing strange, I hope, that men should lie in all ages. You must surely have seen instances enow of that frailty. You have yourself heard many such marvellous relations started, which, being treated with scorn by all the wise and judicious, have at last been abandoned even by the vulgar. Be assured, that those renowned lies, which have spread and flourished to such a monstrous height, arose from like beginnings; but being sown in a more proper soil, shot up at last into prodigies almost equal to those which they relate.

22. It was a wise policy in that false prophet, ALEXANDER, who, though now forgotten, was once so famous, to lay the first scene of his impostures in PAPHLAGONIA, where, as LUCIAN tells us, the people were extremely ignorant and stupid, and ready to swallow even the grossest delusion. People at a distance, who are weak enough to think the matter at all worth enquiry, have no opportunity of receiving better infor-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

mation. The stories come magnified to them by a hundred circumstances. Fools are industrious in propagating the imposture; while the wise and learned are contented, in general, to deride its absurdity, without informing themselves of the particular facts, by which it may be distinctly refuted. And thus the impostor above-mentioned was enabled to proceed, from his ignorant PAPHLAGONIANS, to the enlisting of votaries, even among the GRECIAN philosophers, and men of the most eminent rank and distinction in ROME: Nay, could engage the attention of that sage emperor MARCUS AURELIUS; so far as to make him trust the success of a military expedition to his delusive prophecies.

23. The advantages are so great, of starting an imposture among an ignorant people, that, even tho' the delusion should be too gross to impose on the generality of them *(which, tho' seldom, is sometimes the case)* it has a much better chance for succeeding in remote countries, than if the first scene had been laid in a city renowned for arts and knowledge. The most ignorant and barbarous of these barbarians carry the report abroad. None of their countrymen have a large correspondence, or sufficient credit and authority to contradict and beat down the delusion. Men's inclination to the marvellous has full opportunity to display itself. And thus a story, which is universally exploded in the place where it was first started, shall pass for certain at a thousand miles distance. But had ALEXANDER fixed his residence at ATHENS, the philosophers of that renowned mart of learning had immediately spread, throughout the whole Roman empire, their sense of the matter; which, being supported by so great authority, and displayed by all the force of reason and eloquence, had entirely opened the eyes of mankind. 'Tis true; LUCIAN, passing by chance thro' PAPHLAGONIA, had an opportunity of performing this good office. But, tho' much to be wished, it does not always happen, that every ALEXANDER meets with a LUCIAN, ready to expose and detect his impostures.

24. I may add as a *fourth* reason, which diminishes the authority of prodigies, that there is no testimony for any, even those which have not been expressly detected, that is not opposed by an infinite number of witnesses; so that not only the miracle destroys the credit of testimony, but the testimony destroys itself. To make this the better understood, let us consider, that, in matters of religion, whatever is different is contrary; and that 'tis impossible the religions of ancient ROME, of TURKEY, of SIAM, and of CHINA should, all of them, be established on any solid foundation. Every miracle, therefore, pretended to have been wrought in any of these religions (and all of them abound in miracles), as its direct scope is to establish the particular system to which it is attributed; so has it the same force, though more indirectly, to overthrow every other system. In destroying a rival system, it likewise destroys the credit of those miracles, on which that system was established; so that all the prodigies of different religions are to be regarded as contrary facts, and the evi-

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

dences of these prodigies, whether weak or strong, as opposite to each other. According to this method of reasoning, when we believe any miracle of MAHOMET or his successors, we have for our warrant the testimony of a few barbarous ARABIANS: And on the other hand, we are to regard the authority of TITUS LIVIUS, PLUTARCH, TACITUS, and, in short, of all the authors and witnesses, GRECIAN, CHINESE, and ROMAN CATHOLIC, who have related any miracle in their particular religion; I say, we are to regard their testimony in the same light as if they had mentioned that MAHOMETAN miracle, and had in express terms contradicted it, with the same certainty as they have for the miracle they relate. This argument may appear over subtile and refined; but is not in reality different from the reasoning of a judge, who supposes, that the credit of two witnesses, maintaining a crime against any one, is destroyed by the testimony of two others, who affirm him to have been two hundred leagues distant, at the same instant when the crime is said to have been committed.

25. One of the best attested miracles in all profane history, is that which TACITUS reports of VESPASIAN, who cured a blind man in ALEXANDRIA, by means of his spittle, and a lame man by the mere touch of his foot; in obedience to a vision of the god SERAPIS, who had enjoined them to have recourse to the Emperor, for these miraculous cures. The story may be seen in that fine historian; where every circumstance seems to add weight to the testimony, and might be displayed at large with all the force of argument and eloquence, if any one were now concerned to enforce the evidence of that exploded and idolatrous superstition. The gravity, solidity, age, and probity of so great an emperor, who, thro' the whole course of his life, conversed in a familiar manner with his friends and courtiers, and never affected those extraordinary airs of divinity assumed by ALEXANDER and DEMETRIUS. The historian, a contemporary writer, noted for candour and veracity, and withal, the greatest and most penetrating genius, perhaps, of all antiquity; and so free from any tendency to credulity, that he even lies under the contrary imputation, of atheism and profaneness: The persons, from whose authority he related the miracle, of established character for judgment and veracity, as we may well presume; eye-witnesses of the fact, and confirming their testimony, after the FLAVIAN family was despoiled of the empire, and could no longer give any reward, as the price of a lie. *Utrumque, qui interfuere, nunc quoque memorant, postquam nullum mendacio pretium*. To which if we add the public nature of the facts, as related, it will appear, that no evidence can well be supposed stronger for so gross and so palpable a falsehood.

26. There is also a memorable story related by Cardinal DE RETZ, which may well deserve our consideration. When that intriguing politician fled into SPAIN, to avoid the persecution of his enemies, he passed thro' SARAGOSSA, the capital of ARRAGON, where he was shewn, in

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

the cathedral, a man, who had served seven years as a door-keeper, and was well known to every body in town, that had ever paid his devotions at that church. He had been seen, for so long a time, wanting a leg; but recovered that limb by the rubbing of holy oil upon the stump; and the cardinal assures us that he saw him with two legs. This miracle was vouched by all the canons of the church; and the whole company in town were appealed to for a confirmation of the fact; whom the cardinal found, by their zealous devotion, to be thorough believers of the miracle. Here the relater was also cotemporary to the supposed prodigy, of an incredulous and libertine character, as well as of great genius; the miracle of so *singular* a nature as could scarcely admit of a counterfeit, and the witnesses very numerous, and all of them, in a manner, spectators of the fact, to which they gave their testimony. And what adds mightily to the force of the evidence, and may double our surprize on this occasion, is, that the cardinal himself, who relates the story, seems not to give any credit to it, and consequently cannot be suspected of any concurrence in the holy fraud. He considered justly, that it was not requisite, in order to reject a fact of this nature, to be able accurately to disprove the testimony, and to trace its falsehood, thro' all the circumstances of knavery and credulity which produced it. He knew, that, as this was commonly altogether impossible at any small distance of time and place; so was it extremely difficult, even where one was immediately present, by reason of the bigotry, ignorance, cunning, and roguery of a great part of mankind. He therefore concluded, like a just reasoner, that such an evidence carried falsehood upon the very face of it, and that a miracle, supported by any human testimony, was more properly a subject of derision than of argument.

27. There surely never was a greater number of miracles ascribed to one person, than those, which were lately said to have been wrought in FRANCE upon the tomb of Abbé PARIS, the famous JANSENIST, with whose sanctity the people were so long deluded. The curing of the sick, giving hearing to the deaf, and sight to the blind, were every where talked of as the usual effects of that holy sepulchre. But what is more extraordinary; many of the miracles were immediately proved upon the spot, before judges of unquestioned integrity, attested by witnesses of credit and distinction, in a learned age, and on the most eminent theatre that is now in the world. Nor is this all: A relation of them was published and dispersed every where; nor were the *Jesuits*, tho' a learned body, supported by the civil magistrate, and determined enemies to those opinions, in whose favour the miracles were said to have been wrought, ever able distinctly to refute or detect them. Where shall we find such a number of circumstances, agreeing to the corroboration of one fact? And what have we to oppose to such a cloud of witnesses, but the absolute impossibility or miraculous nature of the events, which they relate? And this

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

surely, in the eyes of all reasonable people, will alone be regarded as a sufficient refutation.

28. Is the consequence just, because some human testimony has the utmost force and authority in some cases, when it relates the battle of PHILIPPI or PHARSALIA for instance; that therefore all kinds of testimony must, in all cases, have equal force and authority? Suppose that the CÆSAREAN and POMPEIAN factions had, each of them, claimed the victory in these battles, and that the historians of each party had uniformly ascribed the advantage to their own side; how could mankind, at this distance, have been able to determine between them? The contrariety is equally strong between the miracles related by HERODOTUS or PLUTARCH, and those delivered by MARIANA, BEDE, or any monkish historian.

29. The wise lend a very academic faith to every report which favours the passion of the reporter; whether it magnifies his country, his family, or himself, or in any other way strikes in with his natural inclinations and propensities. But what greater temptation than to appear a missionary, a prophet, an ambassador from heaven? Who would not encounter many dangers and difficulties, in order to attain so sublime a character? Or if, by the help of vanity and a heated imagination, a man has first made a convert of himself, and entered seriously into the delusion; who ever scruples to make use of pious frauds, in support of so holy and meritorious a cause?

30. The smallest spark may here kindle into the greatest flame; because the materials are always prepared for it. The *avidum genus auricularum*, the gazing populace, receive greedily, without examination, whatever sooths superstition, and promotes wonder.

31. How many stories of this nature have, in all ages, been detected and exploded in their infancy? How many more have been celebrated for a time, and have afterwards sunk into neglect and oblivion? Where such reports, therefore, fly about, the solution of the phænomenon is obvious; and we judge in conformity to regular experience and observation, when we account for it by the known and natural principles of credulity and delusion. And shall we, rather than have a recourse to so natural a solution, allow of a miraculous violation of the most established laws of nature?

32. I need not mention the difficulty of detecting a falsehood in any private or even public history, at the place, where it is said to happen; much more when the scene is removed to ever so small a distance. Even a court of judicature, with all the authority, accuracy, and judgment, which they can employ, find themselves often at a loss to distinguish between truth and falsehood in the most recent actions. But the matter never comes to any issue, if trusted to the common method of altercation and debate and flying rumours; especially when men's passions have taken part on either side.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

33. In the infancy of new religions, the wise and learned commonly esteem the matter too inconsiderable to deserve their attention or regard. And when afterwards they would willingly detect the cheat, in order to undeceive the deluded multitude, the season is now past, and the records and witnesses, which might clear up the matter, have perished beyond recovery.

34. No means of detection remain, but those which must be drawn from the very testimony itself of the reporters: And these, tho' always sufficient with the judicious and knowing, are commonly too fine to fall under the comprehension of the vulgar.

35. Upon the whole, then, it appears, that no testimony for any kind of miracle has ever amounted to a probability, much less to a proof; and that, even supposing it amounted to a proof, it would be opposed by another proof; derived from the very nature of the fact, which it would endeavour to establish. 'Tis experience only, which gives authority to human testimony; and 'tis the same experience, which assures us of the laws of nature. When, therefore, these two kinds of experience are contrary, we have nothing to do but substract the one from the other, and embrace an opinion, either on one side or the other, with that assurance which arises from the remainder. But according to the principle here explained, this substraction, with regard to all popular religions, amounts to an entire annihilation; and therefore we may establish it as a maxim, that no human testimony can have such force as to prove a miracle, and make it a just foundation for any such system of religion.

36. I beg the limitations here made may be remarked, when I say, that a miracle can never be proved, so as to be the foundation of a system of religion. For I own, that otherwise, there may possibly be miracles, or violations of the usual course of nature, of such a kind as to admit of proof from human testimony; tho', perhaps, it will be impossible to find any such in all the records of history. Thus, suppose, all authors, in all languages, agree, that, from the first of JANUARY 1600, there was a total darkness over the whole earth for eight days: Suppose that the tradition of this extraordinary event is still strong and lively among the people: That all travellers, who return from foreign countries, bring us accounts of the same tradition, without the least variation or contradiction: 'Tis evident, that our present philosophers, instead of doubting the fact, ought to receive it as certain, and ought to search for the causes whence it might be derived. The decay, corruption, and dissolution of nature, is an event rendered probable by so many analogies, that any phænomenon, which seems to have a tendency towards that catastrophe, comes within the reach of human testimony, if that testimony be very extensive, and uniform.

37. But suppose, that all the historians who treat of England, should agree, that, on the first of JANUARY 1600, Queen ELIZABETH died; that both before and after her death she was seen by her physicians and the

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

whole court, as is usual with persons of her rank; that her successor was acknowledged and proclaimed by the parliament; and that, after being interred a month, she again appeared, resumed the throne, and governed ENGLAND for three years: I must confess that I should be surprized at the concurrence of so many odd circumstances, but should not have the least inclination to believe so miraculous an event. I should not doubt of her pretended death, and of those other public circumstances that followed it: I should only assert it to have been pretended, and that it neither was, nor possibly could be real. You would in vain object to me the difficulty, and almost impossibility of deceiving the world in an affair of such consequence; the wisdom and solid judgment of that renowned queen; with the little or no advantage which she could reap from so poor an artifice: All this might astonish me; but I would still reply, that the knavery and folly of men are such common phænomena, that I should rather believe the most extraordinary events to arise from their concurrence, than admit of so signal a violation of the laws of nature.

38. But should this miracle be ascribed to any new system of religion; men, in all ages, have been so much imposed on by ridiculous stories of that kind, that this very circumstance would be a full proof of a cheat, and sufficient, with all men of sense, not only to make them reject the fact, but even reject it without farther examination. Tho' the Being to whom the miracle is ascribed, be, in this case, Almighty, it does not, upon that account, become a whit more probable; since 'tis impossible for us to know the attributes or actions of such a Being, otherwise than from the experience which we have of his productions, in the usual course of nature. This still reduces us to past observation, and obliges us to compare the instances of the violation of truth in the testimony of men, with those of the violation of the laws of nature by miracles, in order to judge which of them is most likely and probable. As the violations of truth are more common in the testimony concerning religious miracles, than in that concerning any other matter of fact; this must diminish very much the authority of the former testimony, and make us form a general resolution, never to lend any attention to it, with whatever specious pretence it may be covered.

39. Lord BACON seems to have embraced the same principles of reasoning. "We ought," says he, "to make a collection or particular history of all monsters and prodigious births or productions, and in a word of every thing new, rare, and extraordinary in nature. But this must be done with the most severe scrutiny, lest we depart from truth. Above all, every relation must be considered as suspicious, which depends in any degree upon religion, as the prodigies of LIVY: And no less so, every thing that is to be found in the writers of natural magic or alchimy, or such authors, who seem, all of them, to have an unconquerable appetite for falsehood and fable" [Hume is here quoting Francis Bacon's *Novum Organum*, 2.29].

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

40. I am the better pleased with the method of reasoning here delivered, as I think it may serve to confound those dangerous friends or disguised enemies to the *Christian Religion*, who have undertaken to defend it by the principles of human reason. Our most holy religion is founded on *Faith*, not on reason; and 'tis a sure method of exposing it to put it to such a trial as it is, by no means, fitted to endure. To make this more evident, let us examine those miracles, related in scripture; and not to lose ourselves in too wide a field, let us confine ourselves to such as we find in the *Pentateuch*, which we shall examine, according to the principles of these pretended Christians, not as the word or testimony of God himself, but as the production of a mere human writer and historian. Here then we are first to consider a book, presented to us by a barbarous and ignorant people, written in an age when they were still more barbarous, and in all probability long after the facts which it relates, corroborated by no concurring testimony, and resembling those fabulous accounts, which every nation gives of its origin. Upon reading this book, we find it full of prodigies and miracles. It gives an account of a state of the world and of human nature entirely different from the present: Of our fall from that state: Of the age of man, extended to near a thousand years: Of the destruction of the world by a deluge: Of the arbitrary choice of one people, as the favourites of heaven; and that people the countrymen of the author: Of their deliverance from bondage by prodigies the most astonishing imaginable: I desire any one to lay his hand upon his heart, and after a serious consideration declare, whether he thinks that the falsehood of such a book, supported by such a testimony, would be more extraordinary and miraculous than all the miracles it relates; which is, however, necessary to make it be received, according to the measures of probability above established.

41. What we have said of miracles may be applied, without any variation, to prophecies; and indeed, all prophecies are real miracles, and as such only, can be admitted as proofs of any revelation. If it did not exceed the capacity of human nature to foretel future events, it would be absurd to employ any prophecy as an argument for a divine mission or authority from heaven. So that, upon the whole, we may conclude, that the *Christian Religion* not only was at first attended with miracles, but even at this day cannot be believed by any reasonable person without one. Mere reason is insufficient to convince us of its veracity: And whoever is moved by *Faith* to assent to it, is conscious of a continued miracle in his own person, which subverts all the principles of his understanding, and gives him a determination to believe what is most contrary to custom and experience.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

NOTES

1. No INDIAN, 'tis evident, could have experience that water did not freeze in cold climates. This is placing nature in a situation quite unknown to him; and 'tis impossible for him to tell *à priori* what will result from it. 'Tis making a new experiment, the consequence of which is always uncertain. One may sometimes conjecture from analogy what will follow; but still this is but conjecture. And it must be confessed, that, in the present case of freezing, the event follows contrary to the rules of analogy, and is such as a rational INDIAN would not look for. The operations of cold upon water are not gradual, according to the degrees of cold; but whenever it comes to the freezing point, the water passes in a moment, from the utmost liquidity to perfect hardness. Such an event, therefore, may be denominated *extraordinary*, and requires a pretty strong testimony, to render it credible to people in a warm climate: But still it is not *miraculous*, nor contrary to uniform experience of the course of nature in cases where all the circumstances are the same. The inhabitants of SUMATRA have always seen water fluid in their own climate, and the freezing of their rivers ought to be deemed a prodigy: But they never saw water in MUSCOVY during the winter; and therefore they cannot reasonably be positive what would there be the consequence.

2. Sometimes an event may not, *in itself, seem* to be contrary to the laws of nature, and yet, if it were real, it might, by reason of some circumstances, be denominated a miracle; because, in *fact*, it is contrary to these laws. Thus if a person, claiming a divine authority, should command a sick person to be well, a healthful man to fall down dead, the clouds to pour rain, the winds to blow, in short, should order many natural events, which immediately follow upon his command; these might justly be esteemed miracles, because they are really, in this case, contrary to the laws of nature. For if any suspicion remain, that the event and command concurred by accident, there is no miracle and no transgression of the laws of nature. If this suspicion be removed, there is evidently a miracle, and a transgression of these laws; because nothing can be more contrary to nature than that the voice or command of a man should have such an influence. A miracle may be accurately defined, *a transgression of a law of nature by a particular volition of the Deity, or by the interposition of some invisible agent*. A miracle may either be discoverable by men or not. This alters not its nature and essence. The raising of a house or ship into the air is a visible miracle. The raising of a feather, when the wind wants ever so little of a force requisite for that purpose, is as real a miracle, tho' not so sensible with regard to us.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

References

Ainslie, Donald C. 2015. *Hume's True Scepticism*. Oxford: Oxford University Press.

Allen, James. 2001. *Inferences from Signs: Ancient Debates about the Nature of Evidence.* Oxford: Oxford University Press.

Allison, Henry E. 2008. *Custom and Reason in Hume: A Kantian Reading of the First Book of the Treatise*. Oxford: Oxford University Press.

Anderson, R. Dean. 2000. *Glossary of Greek Rhetorical Terms Connected to Methods of Argumentation, Figures and Tropes from Anaximenes to Quintilian*. Leuven, Belgium: Peeters Publishers.

Andre, Shane. 1993. "Was Hume an Atheist?" *Hume Studies* 19: 141-166.

Aristotle. 1984 [1912-1954]. *Complete Works of Aristotle*. Edited by Jonathan Barnes. Revised Oxford Translation. Princeton: Princeton University Press.

Arnauld, Antoine, and Pierre Nicole. 1996 [1662]. *Logic or the Art of Thinking*. Translated by Jill Vance Buroker. Cambridge: Cambridge University Press.

Averroes (Ibn Rushd). 2008 [1954]. *Averroes' Tahafut Al-Tahafut*. Volume I. Translated and edited by Simon Van Den Bergh. Reprint. London: Gibb Memorial Trust.

Bagger, Matthew C. 1997. "Hume and Miracles." *Journal for the History of Philosophy* 35 (2): 237-251.

Bayes, Thomas. 1763. "An Essay towards Solving a Problem in the Doctrine of Chances." *Philosophical Transactions of the Royal Society* (London) 53: 370-418. Reprinted in *Biometrika* 45 (1958): 296-315. Also reprinted in Earman (2000): 157-176.

Bayne, Steven M. 2007. "Hume on Miracles: Would It Take a Miracle to Believe a Miracle?" *The Southern Journal of Philosophy* 45: 1-29.

Bernoulli, Jakob. 1713. *Ars conjectandi, Opus posthumum. Accedit tractatus de seriebus infinitis, et epistola Gallicè scripta de ludo pilae reticularis*. Basel: Thurnisii fratres.

Bitzer, Lloyd F. 1998. "The 'Indian Prince' in Miracle Arguments of Hume and His Predecessors and Early Critics." *Philosophy & Rhetoric*, 31 (3) (Special Issue: *The Story of the Debate about Miracles*): 175-230.

Broad, C. D. 1916–17. "Hume's Theory of the Credibility of Miracles." *Proceedings of the Aristotelian Society* 17: 77–94.

Brown, Robert. 1987. "History versus Hacking on Probability." *History of European Ideas* 8: 655-673.

Butler, Joseph. 2006. *The Works of Bishop Butler*. Edited by David E. White. Rochester, NY: University of Rochester Press.

Callen, Craig R. 1982. "Notes on a Grand Illusion: Some Limits on the Use of Bayesian Theory in Evidence Law." *Indiana Law Journal* 57 (1): 1-44. https://www.repository.law.indiana.edu/ilj/vol57/iss1/1.

Campbell, Joseph. 1763. *A Dissertation on Miracles: Containing an Examination of the Principles Advanced by David Hume, Esq.; In an Essay on Miracles*. Third edition. Philadelphia: Tomas Dobson. http://www.archive.org/stream/dissertationonmi01camp#page/n5/mode/2up.

Cohen, I. Bernard, and George E. Smith., eds. 2002. *The Cambridge Companion to Newton*. Cambridge: Cambridge University Press.

Cohen, L. Jonathan. 1980. "Some Historical Remarks on the Baconian Conception of Probability." *Journal of the History of Ideas* 41: 219-31.

Coleman, Dorothy. 2001. "Baconian Probability and Hume's Theory of Testimony." *Hume Studies* 27: 195-226.

Coleman, Dorothy. 1988. "Hume, Miracles and Lotteries." *Hume Studies* 14: 328-346.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Collier, Mark. 2005. "A New Look at Hume's Theory of Probabilistic Inference." *Hume Studies* 31: 21-36.
Datson, Lorraine. 1988. *Classical Probability in the Enlightenment.* Princeton, NJ: Princeton University Press.
Dawid, Philip, and Donald Gillies. 1989. "A Bayesian Analysis of Hume's Argument Concerning Miracles." *Philosophical Quarterly* 39: 57-65.
De Pierris, Graciela. 2015. *Ideas, Evidence & Method: Hume's Skepticism & Naturalism concerning Knowledge & Causation*. Oxford: Oxford University Press.
Dees, Richard. 2002. "Morality above Metaphysics: Philo and the Duties of Friendship in *Dialogues* 12." *Hume Studies* 28: 131-147.
Descartes, René. 1985 [1644]. *Principles of Philosophy*, in *The Philosophical Writings of Descartes: Volume 1*, translated and edited by John Cottingham, Robert Stoothoff, and Dugald Murdoch, 177-292. Cambridge: Cambridge University Press.
Domingo, Rafael. 2018. *Roman Law: An Introduction.* New York: Routledge.
Dumsday, Travis. 2008. "Religious Experience: An Unguarded Front in Hume's Account of Miracles." *International Philosophical Quarterly* 48 (3): 371-379.
Earman, John. 2000. *Hume's Abject Failure: The Argument Against Miracles.* Oxford: Oxford University Press.
———. 2002. "Bayes, Hume, Price, and Miracles." *Proceedings of the British Academy* 113: 91-109.
Ellin, Joseph. 1993. "Again: Hume on Miracles." *Hume Studies* 19: 203-212.
Falkenstein, Lorne. 2003. "Hume's Project in *The Natural History of Religion*." *Religious Studies* 39 (1): 1-21.
Ferguson, Kenneth G. 1992. "An Intervention into the Flew/Fogelin Debate." *Hume Studies* 18: 105-112.
Ferguson, Sally. 2002. "Bayesianism, Analogy, and Hume's *Dialogues concerning Natural Religion*." *Hume Studies* 28: 113-130.
Fleetwood, William. 1854 [1701]. *An Essay upon Miracles, in Two Discourses*. Reprinted in Volume 3 of *The Works of the Right Reverend William Fleetwood*. Oxford: Oxford University Press.
Flew, Antony. 1997 [1961]. "The Impossibility of the Miraculous." in *Hume's Philosophy of Belief: A Study of his First Enquiry.* Bristol: Thoemmes Press.
———. 1990. "Fogelin on Hume on Miracles." *Hume Studies* 16: 141-145.
Fodor, Jerry A. 2003. *Hume Variations*. Oxford: Oxford University Press.
Fogelin, Robert. 1990. "What Hume Actually Said About Miracles." *Hume Studies* 16: 81-87.
———. 2003. *A Defense of Hume on Miracles*. Princeton, NJ: Princeton University Press.
———. 2009. *Hume's Skeptical Crisis.* Oxford: Oxford University Press.
Fosl, Peter S. 1999. "Hume, Skepticism, and Early American Deism." *Hume Studies* 25: 171-192.
Fosl, Peter. 1998. "The Bibliographic Bases of Hume's Understanding of Sextus Empiricus and Pyrrhonism." *Journal of the History of Philosophy* 36: 261-278.
Fosl, Peter S. 1994. "Doubt and Divinity: Cicero's Influence on Hume's Religious Skepticism." *Hume Studies* 20: 103-120.
Franklin, James. 2001. *The Science of Conjecture: Evidence and Probability before Pascal*. Baltimore: The Johns Hopkins University Press.
Fréret, Nicholas. 1729. "Réflexions sur l'étude des anciennes histoires et sur le degré de certitude de leurs preuves." *L'Histoire et les mémoires de l'académie royale des inscriptions et belles lettres* 6: 146-189.
Garber, Daniel, and Sandy Zabell. 1979. "On the Emergence of Probability." *Archive for History of Exact Sciences* 21: 33–53.
Garrett, Don. 2002. "Hume on Testimony concerning Miracles." In Millican (2002), 301-334.
———. 1988. *Hume's Philosophy of Religion*. Second edition. London: Macmillan; Atlantic Highlands, NJ: Humanities Press.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

———. 1993. "Hume on Religion." In *The Cambridge Companion to Hume*, edited by David Fate Norton, 313-344. Cambridge: Cambridge University Press.
———. 2003. "*A Defense of Hume on Miracles* by Robert Fogelin." *Philosophical Books* 48 (2): 166-168.
George, Alexander. 2016. *The Everlasting Check: Hume on Miracles*. Cambridge, MA: Harvard University Press.
Gillies, Donald. 2000. *Philosophical Theories of Probability*. London: Routledge.
Glymour, Clark. 1980. *Theory and Evidence*. Princeton: Princeton University Press.
Gordley, James. 2002. "Review of James Franklin, *The Science of Conjecture: Evidence and Probability before Pascal*." *International Journal of Evidence and Proof* 6: 191-194.
Gower, Barry. 1987. "Planets and Probability: Daniel Bernoulli on the Inclinations of the Planetary Orbits." *Studies in History and Philosophy of Science Part A* 18: 441-454.
———. 1990. "David Hume and the Probability of Miracles." *Hume Studies* 16: 17-31.
———. 1997. *Scientific Method: An Historical and Philosophical Introduction*. London: Routledge.
Greenleaf, Simon. 1984 [1846]. *The Testimony of the Evangelists, Examined by the Rules of Evidence Administered in Courts of Justice.* Reprint. Grand Rapids: Baker Book House.
Grieg, J.Y.T., ed. 1983 [1932]. *The Letters of David Hume*. Oxford: Clarendon Press.
Grotius, Hugo. 1625. *De Jure Belli et Pacis*. Paris: Nicalaum Buon.
Haack, Susan. 2014. *Evidence Matters: Science, Proof, and Truth in the Law*. Cambridge: Cambridge University Press.
Haakonssen, Knud. 1990. "Natural Law and Moral Realism: The Scottish Synthesis." In Stewart (1990): 61-85.
Hacking, Ian. 1975. *The Emergence of Probability*. Cambridge: Cambridge University Press.
———. 1978. "Hume's Species of Probability." *Philosophical Studies* 33: 21-37.
———. 1989. "Astronomical Improbability." In *Logic, Methodology and Philosophy of Science VIII*¸ edited by J.E. Fenstad, et al. Amsterdam: North-Holland.
Hájek, A. 2008. "Are Miracles Chimerical?" In *Oxford Studies in Philosophy of Religion*, Volume 1, edited by J. L. Kvanvig, 82-104. New York: Oxford University Press.
Hambourger, Robert. 1980. "Belief in Miracles and Hume's Essay." *Nous* 14: 587-604.
Harper, William. 2002. "Newton's Argument for Universal Gravitation." In Cohen and Smith (2002), 174-201.
Harris, James A. 2015. *Hume: An Intellectual Biography*. Cambridge: Cambridge University Press.
Harris, Michael H. 1966. "David Hume: Scholar as Librarian." *The Library Quarterly: Information, Community, Policy* 36 (2): 88-98.
Harrison, Peter. 1999. "Prophecy, Early Modern Apologetics, and Hume's Argument against Miracles." *Journal of the History of Ideas* 60 (2): 241-256.
Holden, Thomas. 2010. *Spectres of False Divinity: Hume's Moral Atheism*. Oxford: Oxford University Press.
Howson, Colin, and Peter Urbach. 2003. *Scientific Reasoning: The Bayesian Approach*. Chicago: Open Court Publishing Company.
Hume, David. 2013 [1777, 1768, 1748]. *Essays and Treatises on Philosophical Subjects*. Edited by Lorne Falkenstein and Neil MacArthur. Peterborough: Broadview Press.
Hume, David. 1999 [1777, 1768, 1748]. *An Enquiry concerning Human Understanding*. Edited with introduction and notes by Tom L. Beauchamp. Oxford: Oxford University Press.
———. 2000 [1739-40]. *A Treatise of Human Nature*. Edited with introduction and notes by David Fate Norton and Mary J. Norton. Reprinted with corrections 2002. Oxford: Oxford University Press.
Jaynes, E.T. 2003. *Probability Theory: The Logic of Science*. Cambridge: Cambridge University Press.
Johnson, David. 1999. *Hume, Holism and Miracles*. Ithaca: Cornell University Press.
Keynes, John Maynard. 1921. *A Treatise on Probability*. London: Macmillan and Co.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Klibanski, R., and E.C. Mossner, eds. 1954. *New Letters of David Hume*. Oxford: Clarendon Press.
Larmer, Robert A. 2009. "Interpreting Hume on Miracles." *Religious Studies* 45: 325-338.
Levine, Michael P. 2002. "Review of John Earman, *Hume's Abject Failure: The Argument against Miracles*." *Hume Studies* 28: 161-167.
Locke, John. 1963 [1706]. "A Discourse of Miracles." In *Works of John Locke*, Vol. 9. Reprint of 1823 edition printed for T. Teggs, 191-229. Aalen, Germany: Scientia Verlag.
———. 1975 [1690]. *An Essay Concerning Human Understanding*. Edited with an Introduction by N.H. Nidditch. Oxford: Oxford University Press.
Mandelbaum, Eric. 2018. "Troubles with Bayesianism: An Introduction to the Psychological Immune System," *Mind and Language*: 1-17. https://doi.org/10.1111/mila.12205.
Mascardi, Josephi. 1584-88. *On Proofs*. (*Conclusiones probationum omnium* or *De probationibus*.) 3 vols. Venice.
McGrew, Tim. 2005. "Review of *A Defense of Hume on Miracles* by Robert Fogelin." *Mind* 114: 145-149.
Meeker, Kevin. 2013. *Hume's Radical Scepticism and the Fate of Naturalized Epistemology*. Basingstoke: Palgrave Macmillan.
Menochio, Giacomo. 1587-90. *On Presumptions, Conjectures, Signs, and Indications*. (*De praesumptionibus, conjecturis, signis et indicitis*.) Two volumes. Venice.
Middleton, Conyers. 1749. *A Free Inquiry into Miraculous Powers, Which Are Supposed to Have Subsisted in the Christian Church, From the Earliest Ages through Several Successive Centuries*. London: Manby and Cox, on Ludgate Hill.
Millican, Peter. 1993. "'Hume's Theorem' Concerning Miracles." *Philosophical Quarterly* 43: 489-495.
———, ed. 2002. *Reading Hume on Human Understanding*. Oxford: Oxford University Press.
———. 2003. "Hume, Miracles, and Probabilities: Meeting Earman's Challenge." http://www.davidhume.org/papers/millican/2003%20Hume%20Miracles%20Probabilities.pdf.
———. 2011. "Twenty Questions about Hume's 'Of Miracles.'" *Royal Institute of Philosophy Supplement*, Volume 68: 151-192. https://doi.org/10.1017/S1358246111000105
———. 2013. "Earman on Hume on Miracles." https://davidhume.org/papers/millican/2013_Earman_Miracles.pdf.
Montaigne, Michel. 2003 [1588]. *Michel de Montaigne: The Complete Works*. Translated by Donald M. Frame. New York: Alfred A. Knopf
Morris, Ted. 2002. "Commentary on Vanderburgh's 'Of Miracles: Hume's "Abject Failure"' Reconsidered." American Philosophical Association Eastern Division meeting. Unpublished manuscript.
Mossner, E.C. 1980. *The Life of David Hume*. Second edition. Oxford: Oxford University Press.
Mura, Alberto. 1998. "Hume's Inductive Logic." *Synthese* 115: 303-331.
Newton, Isaac. 1999. *The Principia: Mathematical Principles of Natural Philosophy*. Translated by I. Bernard Cohen and Anne Whitman. Berkeley, CA: University of California Press.
O'Brien, Dan. 2007. "A Feminist Interpretation of Hume on Testimony." *Hypatia* 25 (3): 632-652.
O'Conner, David. 2001. *Hume on Religion*. New York, NY: Routledge.
Otte, Richard. 2005. "Review of Robert Fogelin, *A Defense of Hume on Miracles*." *Hume Studies* 31: 165-168.
Owen, David. 1987. "Hume versus Price on Miracles and Prior Probabilities: Testimony and the Bayesian Calculation." *Philosophical Quarterly* 37: 187-202; also reprinted in Millican (2002), 335-348.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

———. 1996. "Philosophy and the Good Life: David Hume's Defence of Probable Reasoning." *Dialogue: Canadian Philosophical Review* 35 (3): 485-504.
Price, Richard. 1768 [1767]. "Dissertation IV, On the Importance of Christianity and the Nature of Historical Evidence, and Miracles." *Four Dissertations*. Second edition. London: A. Millar and T. Cadell. Reprinted in Earman (2000), 157-176.
Reichenbach, Hans. 1951. *The Rise of Scientific Philosophy*. Berkeley & Los Angeles: University of California Press.
Ribeiro, Brian. 2009. "Sextus, Montaigne, Hume: Exercises in Skeptical Cartography." *The Modern Schoolman* 87: 7-34.
Roberts, Paul, and Colin Aitken. 2014. *The Logic of Forensic Proof: Inferential Reasoning in Criminal Evidence and Forensic Science; Guidance for Judges, Lawyers, Forensic Scientists and Expert Witnesses*. Volume 3 of the series Communicating and Interpreting Statistical Evidence in the Administration of Criminal Justice. London: Royal Statistical Society. http://www.rss.org.uk/Images/PDF/influencing-change/rss-inferential-reasoning-criminal-evidence-forensic-science.pdf.
Ruphy, Stephanie. 2011. "From Hacking's Plurality of Styles of Scientific Reasoning to 'Foliated' Pluralism: A Philosophically Robust Form of Ontologico-Methodological Pluralism." *Philosophy of Science* 78 (5): 1212-1222.
Russell, Bertrand. 1948. *Human Knowledge, Its Scope and Limits*. New York: Simon and Schuster.
Russell, Paul. 2004. "Butler's 'Future State' and Hume's 'Guide of Life.'" *Journal of the History of Philosophy* 42: 425-448.
Schliesser, Eric. 2008. "Hume's Newtonianism and Anti-Newtonianism." In The Stanford Encyclopedia of Philosophy (Winter 2008 Edition), edited by Edward N. Zalta. http://plato.stanford.edu/archives/win2008/entries/hume-newton/.
Serjeanston, R.W. 1999. "Testimony and Proof in Early Modern England." *Studies in the History and Philosophy of Science Part A* 30 (2): 195-236.
Shanks, Niall. 2004. *God, the Devil, and Darwin: A Critique of Intelligent Design Theory.* Oxford: Oxford University Press.
Smith, George E. 2002. "The Methodology of the *Principia*." In Cohen and Smith (2002), 138-173.
Snow, Louis F. 1902. "The Education of David Hume." *The Sewanee Review* 10 (2): 207-222.
Sobel, Jordan Howard. 1987. "On the Evidence of Testimony for Miracles: A Bayesian Interpretation of David Hume's Analysis." *Philosophical Quarterly* 37: 166-186.
———. 1991. "Hume's Theorem on Testimony Sufficient to Establish a Miracle." *Philosophical Quarterly* 41: 229-237.
Stewart, M.A., ed. 1990. *Studies in the Philosophy of the Scottish Enlightenment*. Oxford: Oxford University Press.
Stewart, M.A. 1995. "Hume's Historical View of Miracles." In *Hume and Hume's Connexions*, edited by M.A. Stewart and John P. Wright. University Park: Pennsylvania University Press.
Strawson, Galen. 2014 [1989]. *The Secret Connexion: Causation, Realism, and David Hume*. Revised edition. Oxford: Oxford University Press.
Talbott, William. 2011. "Bayesian Epistemology." In The Stanford Encyclopedia of Philosophy (Summer 2011 Edition), edited by Edward N. Zalta. http://plato.stanford.edu/archives/sum2011/entries/epistemology-bayesian/.
Tversky, Amos, and Daniel Kahneman. 1982. "Belief in the Law of Small Numbers." In *Judgements Under Uncertainty: Heuristics and Biases*, edited by Daniel Kahneman, Paul Slovic, and Amos Tversky. Cambridge: Cambridge University Press.
Tweyman, Stanley, ed. 1996. *Hume on Miracles*. Bristol: Thoemmes Press.
Vanderburgh, William L. 2005. "Of Miracles and Evidential Probability: Hume's 'Abject Failure' Vindicated." *Hume Studies* 31: 37-61.
Wooton, David. 1990. "Hume's 'Of Miracles': Probability and Irreligion." In Stewart (1990), 191-229.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

Index

Aristotle, 37, 88, 100–101, 116

Bayesianism, 7, 8, 28, 68, 76, 80–81, 89–90, 105–128, 153, 155, 156–157, 161, 165, 166, 167. *See also* probability, mathematical

causation, 42, 43, 53, 151; regularity theory of, 59–63. *See also* singular causation
Cohen, L. Jonathan, 4, 78, 78–79, 113, 166
Coleman, Dorothy, 4, 6, 36, 68, 77–78, 79, 94–95, 96–97, 113, 123, 166
contrary miracles, 17, 24–26

Datson, Lorraine, 75–77, 86, 87, 91
demarcation criterion, 58, 66, 105–108, 149
diminution principle, Hume's, 35, 67–68, 72, 77, 79, 149–150. *See also* Hume's maxim
Dutch book argument, 7, 79, 117–118, 118–119
De Pierris, Graciela, 36, 46, 52

Earman, John, 1–2, 8, 15, 18–19, 20, 23, 28, 30, 57–58, 67, 68, 77, 81, 87, 105–113, 121–122, 148, 153, 154–155, 157, 160, 161–162, 166
evidence in Roman law. *See* probability, ancient Roman tradition of

Fogelin, Robert, 8, 23, 36, 122, 131, 147–148, 152–162, 166
Franklin, James, 4, 6, 46, 77, 78, 80–84, 86, 89, 91, 93, 99, 113–115, 126
Fréret, Nicholas, 89

Gaskin, J.C.A, 12, 13, 15, 18, 78

Haack, Susan, 120
Hacking, Ian, 6, 78, 88–93, 94, 96–99, 164–165
Hume's maxim, 64–66, 70, 72, 105–108
Hume's own religious position, 14–18

law, ancient Roman, 4, 6, 150. *See also* probability, ancient Roman tradition of
laws of nature, 5, 6, 26, 26–28, 33, 35, 40–41, 43–44, 45–47, 49–63, 72, 119, 157–158, 166; apparently irregular, 49–50, 52, 53, 54, 99, 151; apparent violations of, 50, 56–59, 70, 71, 116, 143, 144–146, 151, 152; probabilistic/statistical, 6, 51–54. *See also* singular causation *and* singular events
Locke, John, 5, 6, 30, 34–36, 38, 40, 47, 63, 76, 79, 89, 98, 100
Logic or the Art of Thinking, 5, 36–41, 47

moral certainty, 7, 30, 75–76, 86, 88, 94, 97, 106, 107–108, 149, 155. *See also* proof, Hume's definition of
miracle, Hume's definition of, 5, 20–21, 23, 26, 50, 146; and invisible miracles, 21; and marvels, 23; not a priori, 18–19, 31, 44, 50–51, 53, 58, 87, 110, 147–148, 150–151, 151, 152–153, 154; not circular, 5, 22–24, 43–45, 53, 161. *See also* contrary miracles
natural religion, 5, 12–13, 15–16, 19, 24, 53, 123–124
Newton, Isaac, 45–47, 115, 156–157
normativism, epistemic, 7, 14, 28, 49, 129–141, 142–144, 159

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

philosophical vs. ordinary levels of analysis, 27, 55, 61, 63, 101, 136–137, 138–139, 140–141
Port Royal Logic. *See* Logic or the Art of Thinking
probability, 29, 41; ancient Roman tradition of, 2, 4, 26, 38, 42, 76, 77, 80–84, 84–89, 89, 133, 167; Hume's species of, 6, 36, 90, 92–93, 94–95, 97, 99, 135; in contemporary law, 34–35, 67, 84, 115, 115–116, 119–120, 126; mathematical/Pascalian/Bayesian, 3, 4, 6, 7, 29, 75–77, 78, 79, 84, 87, 89–90, 96, 108, 112, 113, 114, 117, 126, 152, 156, 157, 163–165, 167; non-mathematical/Baconian/non-Pascalian/non-Bayesian, 3, 4, 6, 28, 76, 77, 78–79, 90, 113, 113–124, 125–126, 127, 152, 155–157, 163, 166–167; of causes in Hume, 52, 95, 155; of chances in Hume, 52, 95, 111, 155; pluralism about, 5, 7, 126–128, 165; unphilosophical, 135, 155, 159. *See also* proof, Hume's definition of
proof, Hume's definition of, 7, 30–31, 34, 36, 77–78, 87–88, 93, 94, 96, 97, 98, 106, 110–111, 112, 122, 148–150, 154, 158, 159, 160, 166

singular causation, 6, 55
singular events, 6, 55, 56–59, 151
skepticism, 7, 101–102; with regard to reason, 131–133, 137–138, 158–160
straight rule of induction, 8, 97, 109–113, 154–155, 155–156, 157, 159, 160, 161–162

testimony, 5, 6, 22, 33–42, 50, 51, 63–64, 65–66, 67, 69, 87, 108, 146, 150, 152, 153–154, 160, 166
Tillotson, John, 21–22
transubstantiation, 21–22

voluntarism, epistemic, 7, 129–131, 136–137, 142–144, 144–146

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use

About the Author

William L. Vanderburgh is professor of philosophy at California State University, San Bernardino, where he teaches a variety of classes including history of philosophy, philosophy of science, logic, and ethics. His research interests are diverse—spanning history of philosophy, philosophy of religion, philosophy of science, and environmental philosophy—but they are unified by a focus on evidential reasoning. He lives in San Diego, California, with his wife and stepdaughter. His craft beer blog, https://CraftBeerInSanDiego.com, where he has reviewed all 200+ San Diego breweries and tasting rooms, won the 2018 BrewDog Beer Blogger Geek and America's Biggest Beer Geek awards. He enjoys racing sailboats on San Diego Bay and taking photographs at the beach. He is currently working on his first novel, a philosophical science fiction comedy involving apprentice gods, multiple universes, and nefarious cats. Vanderburgh is a former Dean of Undergraduate Studies at CSUSB, and he worked as a professor and administrator at Wichita State University for thirteen years before that. He was born in Montreal, grew up in the wilds of Northern Ontario, and earned his Ph.D. at the University of Western Ontario, in London, Canada. He misses Canada, but not the snow.

EBSCOhost - printed on 2/12/2023 8:02 AM via . All use subject to https://www.ebsco.com/terms-of-use